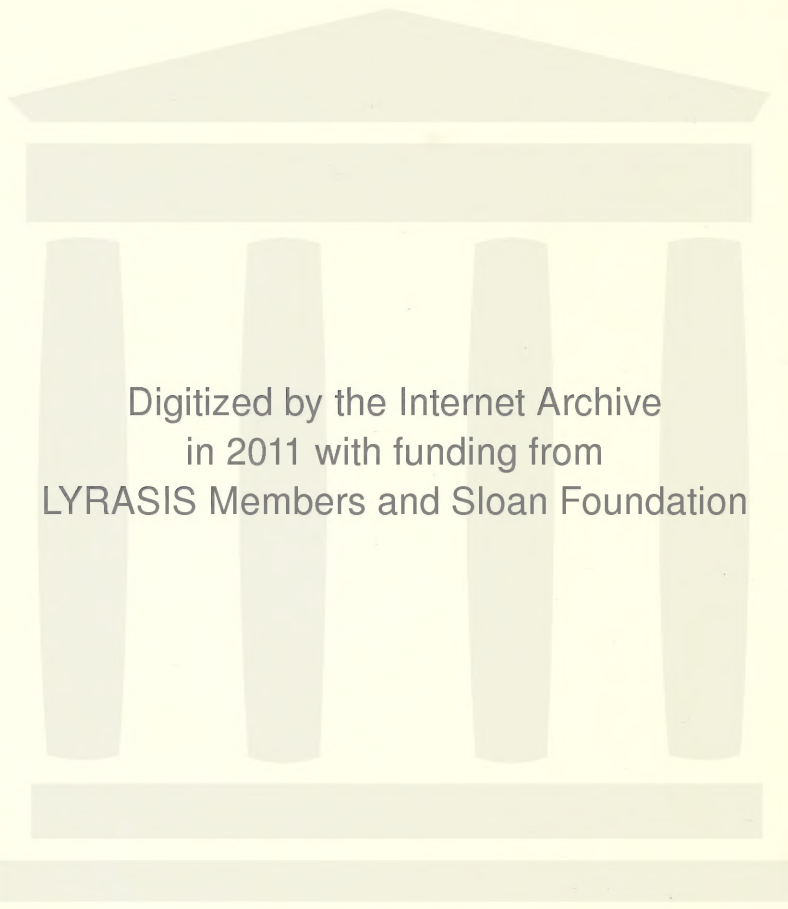


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**LOWIE'S SELECTED PAPERS
IN ANTHROPOLOGY**



LOWIE'S *Selected Papers in Anthropology*

EDITED BY CORA DU BOIS

UNIVERSITY OF CALIFORNIA PRESS

Berkeley and Los Angeles · 1960

*University of California Press
Berkeley and Los Angeles, California
Cambridge University Press, London, England
© 1960 by The Regents of the University of California
Library of Congress Catalog Card Number: 60-8314
Printed in the United States of America
Designed by Marion Jackson Skinner*

Editor's Preface

THE SELECTION OF ESSAYS IN THIS VOLUME HAS BEEN largely determined by Robert Lowie himself. He not only compiled the syllabus of a seminar on his own works (Appendix) but also, during the last ten years of his life, returned again and again to an appraisal of his work and the intellectual climate in which it was pursued. The "Autobiographical Data" in this volume, chapters in *Robert H. Lowie, Ethnologist: A Personal Record* (University of California Press, 1959), and a number of brief notes that he sent to some of his former students testify to the value he set on his various writings. In 1948 I received the following note from him:

An author is notoriously disqualified from properly assessing what he has written. Nevertheless, it is of some psychological interest for others to learn what he himself values as most significant in his output. The award of the Viking Fund medal in December, 1947, prompted me to survey my writings with an eye to determining what I could myself regard as "contributions," i.e., as reasonably "original" and formally acceptable additions to anthropological thinking, as things not said at all, or not so well, by others. The results are meager, but possibly suggestive.

The Plains Indian Age Societies (AMNH-P 11:955-984, 1916)

* Family and Sib (AA 21:28-40, 1919)

* A Note on Aesthetics (AA 23:170-174, 1921)

* Primitive Religion, pp. 3-32, 1924

* A Note on Relationship Terminologies (AA 30:263-267, 1928)
The Crow Indians, pp. 104-118, 1935

* Studies in Plains Indian Folklore (UC 40:1-28, 1942)

* A Case of Bilingualism (Word, 1:249-259, Dec. 1945)

* An asterisk indicates that the paper is included in this volume.

However clear Lowie made the appraisal of his own writing, an editor is nevertheless constrained to exercise selective judgment, particularly when the author's works are as voluminous and diversified as those of Lowie. In this task A. L. Kroeber, Paul Radin, and Leslie Spier gave generously of their advice, and I have included all the articles on which there was consensus. Practical considerations nevertheless remained. While trying to choose articles for their excellence and as illustrations of the range and development of Lowie's thoughts on anthropology, I was limited by considerations of length and availability. Thus, important representative papers that are easily found elsewhere are not reprinted here. Also, the arbitrary decision was made not to reproduce portions of his books and longer monographs. Obviously, no one can understand fully the directions of Lowie's thinking without reference to writings not here included, and above all, to his ten major books and to the eighteen major monographs on the Plains Indians published primarily by the American Museum of Natural History. For example, the omission here of Lowie's contributions to primitive religion represents a serious lacuna that the reader can fill by turning, *inter alia*, to his *Primitive Religion* (1924). This collection therefore cannot pretend to be an intellectual history. I hope, however, that it is a fair sample of Lowie's contributions to anthropology.

In the same vein, another comment on the contents of this volume is warranted. As Kroeber pointed out in his obituary of Robert Lowie written for *Sociologus* (n.s., Vol. VIII, No. 1 [1958], 1-3), "Perhaps a tenth of Lowie's 300 titles—apart from probably some additional 200 reviews—lie outside anthropology in subject—on philosophy, general science, belles-lettres, aesthetics, or biography. A few samples of his interests are Feuerbach, Spencer, Tolstoy, Haeckel, feminism, authority in science, mysticism, Wundt, the golden section, skepticism, bilingualism (he was himself one of the rare complete bilinguals), letters from Mach, parochialism." Although I would not draw the boundaries of anthropology as closely as Kroeber does in this statement, nevertheless it is true that Lowie's wide-ranging interests, and particularly his early interest in philosophy are barely represented even though they are of major importance to an understanding of his sci-

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tific convictions. There was an embarrassment of riches. Since Lowie himself did not include his earlier writings on philosophy among his contributions to anthropology, they are omitted here. Nevertheless, the only two professional photographs that hung over the fireplace in his study at the time of his death were of Mach and Haeckel.

Lastly, in respect to the contents of this volume, the presence of six hitherto unpublished papers should be noted. These were found in Lowie's files after his death in September, 1957. They were made available by Luella Cole Lowie. Although authors are not always well served by posthumous publications of manuscript materials, it was deemed desirable to make these essays available in this volume.

Over and above the question of selection, there remains the question of editing. Lowie does not have the privilege of wielding his own blue pencil as, for example, Kroeber did in *The Nature of Culture* (Chicago: University of Chicago Press, 1952) or as Hallowell did in *Culture and Experience* (Philadelphia: University of Pennsylvania Press, 1955). To presume to perform this function for Lowie would be an impertinence. Therefore the papers brought together here are in the form in which they were first written. Undoubtedly, Lowie would have deleted, revised, and elucidated much that is here reproduced. Had he done so, this volume would have been much improved.

No attempt has been made to standardize the spelling of tribal names, regions, or other words which in Lowie's manuscripts varied over the years. Nor has there been any tampering with certain stylistic idiosyncrasies. Appended lists of references have been omitted in the interest of brevity. The original footnotes in each article have, however, been retained, and some attempt has been made to standardize them for this volume as a whole.

Acknowledgments are always a pleasure. The debt to Kroeber, Radin, and Spier has already been mentioned, although patently final responsibility must rest with the editor. Walter Goldschmidt initiated the idea of this collection of essays, persuaded me to undertake their selection, and made arrangements for their publication. Luella Cole Lowie has been judicious, devoted, and indefatigable in her labors as Lowie's literary executor. Without

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her generous collaboration, my task would have been heavy. Miss Cynthia Nelson of the Department of Anthropology of the University of California, Berkeley, supplied some necessary bibliographical data for the footnotes and Miss Dorothy H. Huggins of the University of California Press undertook the Herculean task of standardizing them throughout the volume. The scholarly benevolence of the editors of various journals who allowed articles to be republished is gratefully acknowledged, and the following publishers and copyright holders are especially thanked for permission to use the material reprinted here: *American Anthropologist*, American Folklore Society, Columbia University Press, The Michigan Academy of Science, Arts, and Letters, National Academy of Sciences, Royal Anthropological Institute, University of Chicago Press (publisher of *American Journal of Sociology*), University of Michigan, *Yale Law Journal*.

To all these Robert Lowie also would have rendered generous thanks. The editor alone must bear the blame for any shortcomings.

CORA DU BOIS

February, 1959
Center for Advanced Study
in the Behavioral Sciences

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Abbreviations in Footnotes

AAA	American Anthropological Association
AMNH	American Museum of Natural History
BAE	Bureau of American Ethnology
PMH	Peabody Museum of American Archaeology and Ethnology, Harvard University
RAI	Royal Anthropological Institute of Great Britain and Ireland
USNM	United States National Museum

Autobiographical Data by Robert H. Lowie

THE CIRCULAR LETTER OF JULY 22, 1943,* CALLS FOR A STATEMENT about "Discoveries which you regard as the most important, and the circumstances under which they were made." Contributions in different fields of knowledge are probably not commensurable; accordingly I feel the need of interpreting the request rather broadly,—as a demand for the writer's subjective evaluation of his more significant scientific activities. For the sake of convenience I shall divide the account under the heads of Field Work; Primitive Social Institutions; Primitive Art and Literature; Primitive Religion; South American Ethnography; Theoretical Position. I shall conclude with some General Remarks.

Field work.—Given primitive cultures that have not yet been adequately described, any trained observer sent to study them is able to make "discoveries." In this respect I was extremely lucky. The first tribe I visited, the Lemhi Shoshone of Idaho (1906), and their cognates in Utah and Nevada, to whom I paid some attention in 1912 and subsequently, were so little known that any

* From the National Academy of Sciences. This paper is Lowie's reply to the circular letter.

tidbit of information was welcome. In consequence the sparse data I presented in *The Northern Shoshone* (AMNH, Anthropological Papers, II [1909], 165–306) and *Notes on Shoshonean Ethnography* (*ibid.*, XX [1924], 187–314) assumed a significance quite disproportionate to their intrinsic worth, for they did assist in more clearly visualizing the relations of the Basin to the Californian and to the Plains areas, respectively. My maiden trip likewise suggested my doctoral dissertation. The casual way in which Lemhi narrators interwove explanatory elements into their tales convinced me that they were not an organic part of the stories. This idea fell in with one of Boas's pet principles, and he encouraged me to elaborate it, whence my first theoretical paper, "The Test Theme in North American Mythology" (*Journal of American Folk-Lore*, XXI [1908], 97–148).

I was also singularly lucky in my studies of the Crow Indians, begun in 1907, continued regularly in the summers from 1910 to 1916, and once more in 1931. Here there had been no information above amateur calibre excepting Lewis H. Morgan's data on the social organization and kinship system. His report of matrilineal clans had been discountenanced in the wave of general skepticism that dominated the turn of the century, but I was soon able to vindicate its accuracy. Further, thanks to the amiability of the Indians as well as to an extraordinarily competent and faithful interpreter, I came to accumulate a good deal of material on social institutions, ceremonies, and religious beliefs. The results appeared in various papers listed in a compact book that summarized them,—*The Crow Indians* (New York, 1935).

Incidental comments, in writing or orally, by Bronislaw Malinowski, Leslie Spier, W. I. Thomas, and others suggest that the data were appreciated from various points of view, perhaps particularly as helping to elucidate the influence of an ideological pattern on individual human behavior. However, I cannot honestly accept Wissler's flattering reference to my work as an example of "approximately complete data" on an aboriginal culture (Clark Wissler, *Man and Culture*, New York, 1923, p. 51), for I am painfully aware of my neglect of the economic and technological aspects of life.

Two phases of my Crow work may perhaps be dignified as "discoveries." Although the literature on American Indians teemed

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with references to visionary experiences, there were never more than a handful from any one tribe,—apart from those recounted in myth. Thanks to the friendliness of my informants, I was able to get a relatively large number of such accounts from Crow visionaries. These, under the stimulus of a passage in Harald Höffding's *The Philosophy of Religion*, I subsequently analyzed in my *Primitive Religion* (New York, 1924) from the point of view of individual differences. As a graduate student I had been fascinated by Galton's *Inquiries into Human Faculty*, and Boas had in his lectures emphasized the variability of human beings in primitive societies. So far as I can see, I went beyond my predecessors in suggesting that the psychological experiences in question probably reflected auditory, kinaesthetic, visual, motor, or other peculiarities of my informants. I do not know to what extent the point attracted notice. If I remember correctly, it appealed to R. R. Marett, who wrote me a commendatory note about the book; and it has recently been cited by Spier ("Franz Boas and Some of his Views," *Acta Americana*, I [1943], 119).

The second "discovery" relates to military organizations of the same people. Earlier reports of corresponding institutions, mainly among neighboring tribes of the Plains, had been worked into Heinrich Schurtz's comprehensive scheme of associations, in which the Crow (and other) military societies represented an evolutionary stage between "age-societies" and "clubs." Brief work among the Assiniboiné Indians in 1908 had made me doubt the age qualification for membership as more than incidental, and among the Crow I systematically pumped every male I could interview concerning his former participation in the organizations under discussion. It thus turned out that within the remembered period the associations investigated were unquestionably not constituted by age-mates. An extension of this line of thought will be noted below.

As a natural corollary to my Crow work, Wissler sent me to the related Hidatsa. Though I never published my notes on them in full, the comparison of these two Plains Indian groups, obviously sprung from a single ancestral tribe some centuries ago, led to various surmises concerning the course of development each had taken since the separation.

Other field work will be referred to under the topical headings.

Primitive social institutions.—My discussion of kinship terminologies was important in the sense that it ushered in a revival of interest in the topic, from which most American students had been alienated by Morgan's evolutionary scheme. The subject had always bored me, but W. H. R. Rivers' *Kinship and Social Organisation* (1914) convinced me that here was a field in which rigorous formulation of problems was possible. Among other things, Rivers had suggested a correlation between a clan system and one type of "classificatory" kinship nomenclature, an idea anticipated by Tylor with reference to moieties. Rivers' conception rested on Oceanian data; I examined available North American information, confirming Rivers' general point ("Exogamy and the Classificatory System of Relationship," *American Anthropologist*, XVII [1915], 223–239.¹ Finding enormous gaps in the relevant data, however, I turned propagandist, advertising the significance of the problem on every occasion. The result was the accumulation of much trustworthy material by Kroeber, Sapir, Speck, Elsie Clews Parsons, Gifford, Spier, Lesser, and others. Specifically, the Southwestern terminologies, hardly known before, came to be described in great abundance.

My own role in this development, apart from that of promoter, was a restricted one. I grew more open-minded concerning the part played by the levirate and sororate (championed by Sapir) and tried to link the origin of the North American clans with particular groupings of kin ("Family and Sib," *American Anthropologist*, XXI [1919], 28–40).² Under the category of "discovery" I should place my findings on the system of the Hopi Indians. Since this tribe was the only Shoshonean people with a clearly developed clan organization, they provided an ideal field for testing Rivers' theory. It appeared that they really differed radically from their clanless congeners and precisely so as to suggest a correlation with the clan structure (*Hopi Kinship*, AMNH, Anthropological Papers, XXX [1929], 361–388).

My personal contributions, however, remained of limited scope. What attracted me were the functional relations and to some extent historical connections suggested by kinship terminologies. I

¹ Reprinted as No. 2 in this volume.

² Reprinted as No. 5 in this volume.

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never did what might reasonably have been expected of me, viz., bring Morgan up to date even for a single major area, as Gifford did for California. Radcliffe-Brown, Kroeber, Gifford, Lesser, Warner, and Aginsky have all probably been more deeply interested in the relevant facts than I ever was. It strikes me as characteristic that I turned to the conceptual aspect of the field, proposing a new typology of kinship systems to supersede Morgan's and Rivers' ("A Note on Relationship Terminologies," *American Anthropologist*, XXX [1928], 263-267).³ Its essential soundness seems indicated by the fact that soon after its publication Kirchhoff independently elaborated a virtually identical scheme. It also stimulated some more elaborate attempts by Warner and Aginsky.

By and large, my work on the Plains Indian military organizations probably constitutes my most important "discovery." As stated, it grew out of my early field work and a critical reading of relevant theories. Coupled with my doubts concerning the age factors was the conclusion that here, too, Boas's principle of secondary association could be abundantly demonstrated. These ideas were adumbrated in my descriptive report on *The Assiniboine* (AMNH, *Anthropological Papers* [1909], 75-98). They aroused favorable comment from Sapir and others, and Clark Wissler was stimulated to organize one of the Museum's major projects, a systematic study of Plains societies in which virtually all members of the staff participated in some measure for half a dozen years. The results were embodied in Volume XI of the Museum series, the concluding section "Historical and Comparative Summary" (New York, 1916) being naturally entrusted to me. This paper met with more favor than any other of my technical writings. Boas, generally chary of praise, told me that it seemed to be "exceedingly well done," and it is the only one of my publications, I think, of which Goldenweiser wholeheartedly approved.

In retrospect I should discriminate. There is, for my present taste, too much of what Radcliffe-Brown calls "conjectural history," and to many of the specific chronological hypotheses I now attach little importance. Nevertheless some of the historical

³ Reprinted as No. 6 in this volume.

results still appear well grounded. I established a closer relation at one time between the Arapaho and the Village tribes than appears from documentary evidence; and I demonstrated the identity of the Arapaho Tomahawk society with the Lumpwoods of the Hidatsa. However, the principal conclusions seem to me to lie in other directions. I think I proved that during the historical period the very organizations which most clearly seemed to exemplify Schurtz's age-sets did not at bottom correspond to particular age-levels, membership primarily hinging on payment of a fee. They were age-classes only because age-mates tended to buy membership jointly; without the payment entrance was impossible, and on the other hand a man who had purchased his membership retained it irrespective of age until he had sold what were basically property rights. Thus, the age factor turned out to play a very different part from that in Schurtz's scheme. Secondly, in contravention of unilinear schemes of evolution, I showed that the superficially similar age-sets of the Masai and clubs of Melanesia were radically different from the Plains Indian associations, each area having produced a phenomenon *sui generis*.

Before leaving the subject of Social Organization I feel impelled to comment on my *Primitive Society* (New York, 1920), whose success illustrates that an opportune publication may create an effect quite disproportionate to its merit. There were indeed a few voices that damned with faint praise. Kroeber (*American Anthropologist*, XXII [1920], 377-381) commended its honesty, but emphasized the lack of "broad results," the "comparative sterility," want of "visions of more ultimate enterprise." Rivers (*ibid.*, 278-283) criticised my overcautious and "mechanical" use of diffusion and rightly pointed out my ignorance of Oceanian and African data. But the discordant voices were drowned in a chorus of acclamation. Boas, meeting me soon after the book had appeared, said, "I think you have written an awfully good book"; Sapir, after writing me that he was "fairly enthusiastic" about it, published laudatory reviews in *The Nation*, *The Freeman* and *The Dial*; Laufer and Wissler expressed themselves orally in eulogistic terms; Elsie Clews Parsons hailed the book in *The New Republic*. John M. Cooper even now rarely sees me without making favorable comments on the work and urging me

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to bring it up to date. Malinowski was consistently laudatory in oral and published statements; even Goldenweiser went so far as to call it "a good book." Father Wilhelm Schmidt was eager to have it translated into German, a project thwarted only by the Austrian financial conditions at the time. On the other hand, Payot was willing to publish Mme E. Métraux's translation fifteen years after the original edition under the title of *Traité de sociologie primitive*; and subsequently there was even a Japanese translation.

From a sordid point of view, *Primitive Society* was the only book I ever wrote except for my elementary *Introducton to Cultural Anthropology* (1934; 1940) that proved profitable. Whereas some others failed to recoup me for typing expenses, six thousand copies of *Primitive Society* were sold in the United States; and as early as 1924 the royalties paid for a trip to Europe.

Looking at the book objectively, I recognize more defects than the severer critics have brought out. My preparation for the task I set myself was wholly inadequate. Apart from the problems of associations and of the North American kinship and clan systems I had not been particularly interested in social organization. Moreover, as a museum worker I had not had an opportunity to organize my ideas on relevant questions except during a semester course as a visitor at the University of California. In consequence I had never considered many vital questions when I set pen to paper and was grossly ignorant even of what was then ascertainable concerning African and Oceanian phenomena. Having rashly promised my then publisher to furnish the manuscript within four months, I very imperfectly filled the gaps in my knowledge during that span of time.

Why, then, the general recognition by the crowd? Several factors have to be considered here. First, there had been no general treatise since Morgan's *Ancient Society* (1877). Since that date field research had accumulated vast stores of intrinsically interesting material, of which my book, however inadequately, gave the reader some conception. It afforded some insight into the phenomena of social units based on kinship, such as had been treated by Morgan, and also those first systematically dealt with by Schurtz. Experience seems to show that a reader is always

grateful for his first orientation in a major field, and my book profited from the lack of any handbook on primitive sociology that was at all up-to-date in its standpoint. Secondly, *Primitive Society* embodied very largely not my original conceptions, but the anti-unilinear and diffusionist views that had gained momentum in both Europe and America. Specifically, my criticism of older views fell in with the picture of early family life as conceived by Boas, Swanton, Malinowski, Schmidt, and others; I happened to be the first, not to voice them, but to voice them in a *general* work, thus accidentally appearing as the spokesman of an important body of anthropological thinkers. Thirdly, I naturally incorporated in the book the conclusions I had previously set forth concerning Plains Indian associations and relationship terms in technical papers. These had naturally had a limited public, thus whatever merit belonged to them was attached by most readers to the general book.

One topic that, I believe, was really advanced in *Primitive Society* was that of property rights. That they existed on even very primitive levels was, in my opinion, established in the relevant chapter; which has been specially praised by such sociologists as Franklin H. Giddings and William F. Ogburn. Subsequently I slightly enlarged on one aspect of the subject in an article on "Incorporeal Property in Primitive Society" (*Yale Law Journal*, XXXVII [1928], 551-563).⁴

Primitive art and literature.—My observations on primitive art in general merely followed the lines traced by my predecessors, —Boas, Wissler, and Kroeber, though possibly in the study of Plains Indian rawhide bags I more than earlier students stressed the total configuration of the decoration rather than its constituent elements (*Crow Indian Art*, AMNH, Anthropological Papers, XXI [1922], 271 ff.). However, I attach greater importance to exact measurements of comparable designs in neighboring tribes ("A Note on Aesthetics," *American Anthropologist*, XXIII [1921], 170-174).⁵ Though wholly neglected except by Spier, this paper bears on the psychology of aesthetic perception and points the way toward a more refined comparative technique than is com-

⁴ Reprinted as No. 18 in this volume.

⁵ Reprinted as No. 10 in this volume.

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monly in vogue. It was stimulated by Gustav Theodor Fechner's speculations on the "golden section" and the ideal rectangle.

Apart from routine recording of aboriginal tales, partly in the original, and indicating their historical connections, I have concerned myself with stylistic questions and the problem of the native narrator's creative processes. Specifically, I have dealt with the degree of accuracy with which a traditional text is reproduced (*Studies in Plains Indian Folklore*, Univ. Calif. Publ. Amer. Arch. and Ethn., XL [1942], 1-28).

Religion.—My conception of ceremonialism as largely a combination of spectacular non-religious elements secondarily associated with the religious aspects ("Ceremonialism in North America," *American Anthropologist*, XVI [1914], 602-631)⁶ was a logical extension of Boas's principle of secondary association, already exemplified in my doctoral dissertation with reference to mythology and later in the study of Plains societies.

My general treatment of the entire subject in *Primitive Religion* met with a curious variety of responses. One of the oddest features about them is a disparity on one point. Goldenweiser announced in a review that I had merely dealt with the paraphernalia of religion, Sapir wrote me in corresponding fashion, Radin expressed himself similarly to others, and Ogburn has recently pronounced it "singularly defective in giving the reader any feeling for the spirit of religion" (Wm. F. Ogburn and Meyer F. Nimkoff, in *Sociology*, Cambridge, 1940, pp. 69 ff.). Considering the uncanny concern of these gentlemen for the interests of religion, I note the absence of this particular line of criticism among its official representatives, Archbishop N. Söderblom, Father Wilhelm Schmidt, Monsignor J. M. Cooper. Professor Robert L. Calhoun of the Yale Divinity School and one of his colleagues whose name has slipped my memory have orally expressed their satisfaction with the book.

As to my "contributions" in this field, my own opinion is as follows. So far as I know, I was the first ethnographer to attempt a tie-up between the ethnographic data and scientific psychology. Specifically, I emphasized the occurrence of individual variability, and not merely with regard to visionary experiences. Fur-

⁶ Reprinted as No. 24 in this volume.

ther, I suggested several problems for future research, such as the role of women in primitive religion and the nature of the ideas that are associated by primitive man in his beliefs and cults.

My position toward religion as a cultural phenomenon springs directly from my conception of anthropology as a *science*. A science of culture must take cognizance of values because these form an essential part of its subject-matter, but it must treat them *objectively*. The technique I use for doing this is verbatim quotation: The ineffable cannot be described by wordy ravings of an expositor, which must be "singularly defective in giving the reader any feeling for the spirit of religion." But the utterances of the religious person himself will come as close as anything can to suggesting what the thrill of ecstasy and other phenomena of faith are like; they are certainly more authentic than anything Goldenweiser or Benedict or anyone else might say on the subject.

Further, an anthropological point of view must steer clear of judging the values thus set forth and must consistently maintain its benevolent neutrality not only in dealing with West African fetichism, but even in the face of Roman Catholicism or Greek Orthodoxy. Some of the most eminent anthropologists have failed to attain this point of view, and for myself it was not easy to rise above the encyclopaedist attitude of my early manhood. It was the second section of chapter iv in Mach's *Die Mechanik* (Leipzig, 1912), pp. 429 ff., that roused me from my skeptical inertia and made me see the Church in proper perspective as against the force of traditional thinking quite apart from ecclesiastical doctrine. The substitution of a genial attitude toward contemporary faiths for my earlier anticlericalism occurred between 1918 and 1924; it has been a persistent part of my philosophy; and ever since, as in my popular book *Are We Civilized?* (New York, 1929), I have been fond of pointing out in the interests of fairness the irrationalism of non-clerical writers. From oral and printed statements of my elders and contemporaries I infer that in this position, which I regard as the only justifiable one scientifically, I stand very nearly alone in these age-classes.

For the meager appreciation of my book there were both intrinsic and accidental reasons. For one thing, it did not survey the entire field systematically, as *Primitive Society* in principle

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had surveyed its own. Secondly, I now appeared not as the spokesman of a group, but as a free-lance reporting on his adventures into by-paths that had lured him. It was too much to expect that many readers, even if otherwise kindly disposed, should share my own predilections. Thus, Father Schmidt reviewed *Primitive Religion* favorably enough, but with reference to the problems that interested *him*, not me, in this connection; the parts I attached importance to, i.e., those concerned with psychology, he passed over with perfunctory commendation while lavishing much space on historical questions which I had explicitly declared as of minor significance from my point of view. According to less benevolent critics—Goldenweiser and his following, e.g.—I was getting above myself in daring to write on a subject too subtle for my pedestrian comprehension. Though I do not accept this judgment as objectively valid, I am deeply sympathetic with it as a subjective biological reaction. Intuitively sensing my appraisal of their scientific worth, the critics naturally resented it; hence also my approach to scientific problems. In maturer years I found solace against this form of detraction in Goethe's reflections on Kotzebue; at a less serene stage I confess that my sentiments were nearer those of Goethe's Götz von Berlichingen in a famous unquotable quotation.

South American ethnography.—In 1926 the late Baron Erland Nordenskiöld first called my attention to Curt Nimuendajú of Belem do Pará as an exceptionally fine ethnographer, but I was then little interested in South America. Subsequently Norden-skiöld's student, Dr. Itzikowitz, asked whether something could be done for Nimuendajú, who himself wrote me a letter and sent a sample manuscript. In 1935 I was able to get him a grant for field work from the Institute of Social Sciences of the University of California and to have this renewed for a number of years, sometimes eked out or superseded by grants of other institutions. In consequence Nimuendajú revisited various Gê tribes and, more recently, the Tukuna, presenting possibly the best all-round studies in existence on the simpler South American tribes. These I translated from his German manuscripts and, so far as possible, published in English translation after a copious correspondence conducted by both of us in German. *The Apinayé* (The Catholic

University of America, Anthropological Series, No. 8, Washington, D.C., 1939) and *The Serente* (Publications of the Frederick Webb Hodge Anniversary Publication Fund, Vol. IV, Los Angeles, 1942) are the most substantial publications up to date, but a far more comprehensive treatise, "The Eastern Timbira," which is to appear in the University of California series, has just reached the galley proof stage (February, 1944).⁷ A sizable Tukuna monograph remains untranslated.⁸

The extraordinary accuracy of Nimuendajú's observations, which revealed unexpected phenomena in the sociological and religious culture of South American tribes, stimulated a number of brief papers setting forth their theoretical import for the reconstruction of American culture history or for the problems of culture growth generally. The point I especially emphasized was the remarkable tendency of the simple Gê tribes to strike out along independent paths from an obviously common cultural base ("A Note on the Northern Ge Tribes of Brazil," *American Anthropologist*, XLIII [1941], 188-196).

My acquaintance with Nordenskiöld led to another successful bit of promoting. When I was in Sweden in 1924, he and Rivet had told me of a Handbook of South American Indians which they had in mind as a joint venture. In 1932, when hard put to it to justify to my conscience my drawing a salary as Chairman of the Division of Anthropology and Psychology of the National Research Council, I proposed that the Council foster a Handbook of the sort indicated and ask Nordenskiöld to edit it. He was favorably inclined, but died soon after. For years nothing happened except that the Division continued a committee concerned with the project and that the Smithsonian Institution assumed moral sponsorship. However, when Latin-American relations loomed on the political horizon, Congress was persuaded to make an appropriation and the State Department came to take a live interest in the work as tending to promote pan-American amity. Dr. Julian H. Steward of the Smithsonian, one of my former stu-

⁷ Curt Nimuendajú, *The Eastern Timbira*, Univ. Calif. Publ. Amer. Arch. and Ethn., Vol. XLI (1946).

⁸ Translated by William D. Hohenthal and edited by Robert H. Lowie, *The Tukuna* was published in 1952 as Univ. Calif. Publ. Amer. Arch. and Ethn., Vol. XLV.

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dents and also of Nordenskiöld's during his stay at Berkeley, became editor in chief and successfully rallied many contributors. As a result the material for the *Handbook* has been accumulating at a terrific rate. Printing on the first volume is to begin at the time of writing (February, 1944).

Theoretical position.—My general attitude toward cultural anthropology has consistently been that it is a science which requires precisely the same logical and psychological processes as any of the supposedly more exact sciences. That our subject matter also permits artistic treatment I have never denied, but I have tried to keep apart objective findings and aesthetic interpretation. My attitude toward certain modern fashions in anthropology should be considered in this light. I do not necessarily deny all significance to them, but I do not care to take up with them until I see some likelihood for objective procedure. This holds especially for the attempted synthesis of psychoanalysis and anthropology.

In the traditional domain of anthropology I have been a "middle-of-the-road" man. That is, I have, like Tylor and Boas, accepted both diffusion and independent development according to the evidence in particular cases. Much more than Boas I have maintained a faith—not in "laws," indeed—but in the possibility of determining fruitful correlations, especially in the study of social structure.

PART I *Kinship and Social Organization*

The eight papers included here form only a minor part of Lowie's extensive publication on kinship and social organization. They serve nevertheless to illustrate the kinds of problems that concerned him and the critical acumen he brought to them. They serve also to illustrate his ever-widening command of ethnographic information, his tenacious preoccupations, and his willingness to admit error when convinced by careful rescrutiny of his position.

1

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LIKE THE GENERATION OF THINKERS THAT PRECEDED OURS, we are living in an age of revolt, but the object of our revolt is different from theirs. Our predecessors fought tradition as arrayed against reason. We have the task of exorcising the ghosts of tradition raised in the name of reason herself. There is not only a folklore of popular belief, but also a folklore of philosophical and scientific system-mongers. Our present duty is to separate scientific fact from its envelope of scientific folklore. This duty has been recognized by workers in various fields. And so we have in philosophy James's protest against monistic mythology; in physics and chemistry Mach's protest against mechanistic mythology; in biology and anthropology a no less vigorous protest against evolutionary mythology. Monism, mechanism, evolution are doubtless valuable concepts; but they are valuable in proportion as they are free from scientific folklore.

Our present course of lectures is designed to help in the separation of anthropological fact from anthropological folklore. This is the more necessary because not only laymen but even scientists

Two lectures (in a course of four) delivered in January under the auspices of the Department of Anthropology, American Museum of Natural History. Printed in *American Journal of Sociology*, XX (July, 1914), 68-97.

of neighboring fields—historians, economists, sociologists, social reformers—continue to use as definitely established truths anthropological theories that are now gracing only the refuse heaps of the modern anthropologist's laboratory.

In my own lectures, I will confine my attention to a single problem—that of the clan and the gens, or, to call both by a generic term, that of the one-sided exogamous kinship group, a group that traces descent either through the father or through the mother exclusively, and within which marriage is prohibited (exogamy). I will outline the conception of this group in older anthropological literature. I will show why we secessionists revolt against that conception. And I will attempt to show how nowadays we grapple with the range of facts that concept was intended to summarize.

The concept we are here concerned with has been most clearly defined by Lewis H. Morgan in his *Ancient Society*, a work that has molded the views of innumerable professional and non-professional students of anthropology on the social organization of primitive tribes. This is not the place to define accurately Morgan's place in the history of anthropology. To avoid misunderstanding, I will state at the outset that in my opinion that place will remain a high one. But we must distinguish between Morgan the observer, and Morgan the theorist; and in Morgan's theoretical work we must again distinguish between his unusual power to see the importance of certain facts that had escaped others, and the very ordinary power shown in his naïvely synthetic constructions. Morgan's observations have indeed been challenged in part, yet in almost every instance, not only with reference to the Iroquois but also as regards tribes he was less intimate with, they have been corroborated by later and more thorough investigation. We are, therefore, entitled to consider him a painstaking, trustworthy observer. On the other hand, Morgan's interpretation of human society as a whole was not only unduly colored by his personal observations among the Iroquois, but reflected the trend of his age toward artificial evolutionary schemes. To develop such a scheme requires more than average ability, but, contrary to current notions, it does not require a very high grade of ability,

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certainly not of scientific ability. This Morgan displayed in a far more convincing manner when he noted the character of the Iroquois kinship system as distinct from our own, defined it, and set about with truly Darwinian industry to determine its analogues the world over. This genuinely scientific and theoretically important undertaking was doubtless not so spectacular as the interpretative speculations he superadded to the facts, but it will be rated higher by future generations.

To attack our problem. In *Ancient Society* Morgan's general aim is to trace the history of social organization from the period of savagery to that of latter-day civilization. This development, he contends, took place through a series of unconscious reformatory movements enforced by natural selection. Low down in the scale of savagery there was a period of intermarriage of brothers and sisters in a group. At a later stage this was prevented by forming social units that would include brothers and sisters (as well as many other members of the tribe), and prohibiting marriage between all members of the new units. These organizations were of two distinct types according to whether kinship was traced through the mother or the father: they were either what are now known as "clans" or what we now call "gentes." A clan consists of "a supposed female ancestor and her children, together with the children of her female descendants, through females, in perpetuity." A gens consists of "a supposed male ancestor and his children, together with the children of his male descendants, through males, in perpetuity." Both the clan and the gens would bar intermarriage of brothers and sisters, and also marriage of cousins, no matter how distant, belonging to the same kinship group. On the other hand, marriage was not thereby prevented between all blood-relatives. With female descent, for example, I should not be permitted to marry my mother's sister's daughters, because they belong to my own clan, but I might marry my father's sister's daughters, who would necessarily belong to another clan. Morgan believes that, once invented, the scheme of the one-sided exogamous kin group spread "over immense areas through the superior powers of an improved stock thus created." With the exception of Polynesia, it formed "the nearly universal

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plan of government of ancient society, Asiatic, European, African, American, and Australian.”¹

To this notion of the one-sided exogamous kin group Morgan added a theory of how that group developed from an archaic to a relatively modern form. In order to secure “the benefits of marrying out with unrelated persons,” it would obviously be immaterial whether kinship is reckoned on the mother’s or father’s side, so long as an equal number of relatives were prevented from mating. But Morgan holds that at the time when the one-sided exogamous group originated “marriage between single pairs was unknown, and descent through males could not be traced with certainty” (p. 67). Hence, he contends, in the archaic form of the kin group, kinship could be reckoned only in the maternal line, which also determined inheritance. When the paternity of children was assured, Morgan assumes that fathers revolted at their children being disinherited by the clan rule of inheritance, and in this way descent in the female line was overthrown and patrilineal descent substituted: in modern terminology, the clan was changed into a gens. Such, at least, is Morgan’s account for the change among the Greeks and Romans (p. 345). For the Indian tribes with gentes he does not venture to suggest throughout the same motive for the development of the gens from the clan. Speaking of the Siouan family, he writes:

It is surprising that so many tribes of this stock should have changed descent from the female line to the male, because when first known the idea of property was substantially undeveloped, or but slightly beyond the germinating stage, and could hardly, as among the Greeks and Romans, have been the operative cause. It is probable that it occurred at a recent period under American and missionary influences (p. 157).

In general it may fairly be said that Morgan regards descent traced through the father as a quite recent institution, and believes in the ancient universality of the clan among North American Indian tribes (e.g., p. 177).

¹ L. H. Morgan, *Ancient Society* (New York, 1877), pp. 27, 63, 74, 377–379. I am using the term “gens” not in Morgan’s sense but in that now common among American ethnologists.

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It is my intention to test Morgan's theory by the Indian data on which it is primarily founded. We may begin our test with the two most tangible questions that develop in connection with Morgan's scheme. Was the one-sided exogamous kinship group really a universal institution among the natives of North America? And did the exogamous gentes found among them develop uniformly out of exogamous clans? These questions have been answered by Dr. Swanton,² in the light of modern investigation, and while still later research has corrected his statement of the case in detail I find myself in full agreement with his general conclusions.

In answering the first question it would not serve our purpose to enumerate the tribes that have exogamous kinship groups and set off against them the tribes that have not. For in this manner the real meaning of the facts would often be obscured through lack of weighting. For the question of the ancient universality of the exogamous kinship group is not equally significant whether the institution occurs among two quite unrelated tribes or among two tribes which, like the Hidatsa and Crow or some of the Southern Siouan tribes, have only branched out from a common ancestral tribe during the last four or five centuries. And obviously the recent adoption of a clan or gentile system, which in a fair number of instances is demonstrably a result of borrowing from neighboring tribes, is of no importance from this particular point of view. Fortunately the essential facts can be expressed in a somewhat summary fashion, owing to the geographical continuity of the tribes possessing the system in question. We find it, roughly speaking, in the greater part of the United States, east of the Mississippi, and some of the adjoining Canadian territory; among the Caddo and Southern Siouan tribes of the Plains, as well as among several of the Northwestern peoples in the same area; in New Mexico and Arizona; on the coast of British Columbia and Alaska and in part of the Northwest coast hinterland. There are thus four fairly continuous areas within which the one-sided exogamous kin group is known to exist. In the remaining part of North America north of Mexico no such institution has

² John R. Swanton, "The Social Organization of American Tribes," *American Anthropologist*, VII (1905), 663-673.

been discovered. Among the Eskimo, throughout the Mackenzie River and Plateau areas, as well as in nearly all of California and several of the Plains tribes, diligent inquiry has failed to reveal any trace of such an exogamous system. It cannot be supposed that the system did exist in these regions but has escaped the notice of observers. For there is nothing esoteric about the rule that kin must not intermarry; and where exogamous groups occur the social activities connected with them are so prominent that, according to the experience of American field workers, their existence is very readily ascertained. The supporters of Morgan's views must, therefore, reckon with the fact that the supposedly universal organization simply does not exist in a very large part of North America. It might be asserted that the system had once existed everywhere, but that in certain districts it has disappeared. But this remains a baseless assertion in the absence of any proof that such a process has occurred and in the absence of any reason for such a process of degeneration in the regions concerned. It cannot be maintained either that the tribes in question have advanced beyond the clan or gentile stage. When we compare the culture of the Shoshone, Paiute, Thompson River Indians, and others lacking the one-sided exogamous kin group with the Pueblo Indians, Iroquois, Omaha, and others possessing the system, it is at once apparent that whether from the point of view of industrial arts, social life, or ceremonial activity the tribes possessing the system are the more advanced. Swanton has rightly emphasized the fact that almost all the tribes with a clan or gentile organization are agriculturists, while the rest are almost all non-agricultural. The weight of such considerations as these has led Frazer—in other ways a typical representative of the classical school in anthropology—to reject Morgan's position and to admit that the stage of the exogamous clan or gentile system had never been attained by "the more backward members of the Redskin family."³

This statement of the facts must not, however, be interpreted to mean that the tribes in question represent a stage preceding that of the clan in Morgan's scheme. For in that case we should expect no definite restriction of marriage, even between own broth-

³ J. G. Frazer, *Totemism and Exogamy* (London, 1910), III, 1-3.

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ers and sisters,⁴ while on the contrary we almost uniformly do find additional restrictions based on consanguinity. To cite only a few random examples: Among the Central Eskimo, marriages of cousins, nephews and aunts, nieces and uncles are prohibited.⁵ Cousins are forbidden to marry among the Thompson River Indians, and even second-cousin marriages are disapproved.⁶ In the Nez Percé tribe there were no restrictions of marriage except in the case of relatives, but even second or third cousins were not allowed to marry.⁷ In these cases, of course, a superadded one-sided exogamous kin system would not "secure the benefits of marrying out with unrelated persons," because these benefits are already secured by existing marriage restrictions based on bonds of consanguinity. Thus the tribes in question in no way fit into Morgan's scheme of social evolution. They are not more advanced than the tribes possessing exogamous kin groups, for their general culture is undoubtedly lower. But neither are they so low in their social customs as to require an exogamous kin system for the retrenching of consanguine marriages. If anything, we should have to say that in this particular point they are higher, that is, nearer to our own mode of conduct, than the tribes organized in exogamous kin groups as conceived by Morgan.⁸ It might still be argued that tribes may advance very unequally in different departments of culture; that therefore the loosely organized peoples may have lagged behind in their economic and industrial life while forging ahead of the tribes with clans or gentes in their social usages; that therefore they did once possess clans or gentes but have passed beyond that stage. The general principle on which such an argument would rest is sound, but its application is highly unconvincing in the present case. It would never be applied except to save the endangered hypothesis, involving as it does an appeal not to any observable facts, but to our ignorance of unobservable ones. Accordingly, we may dismiss it and

⁴ Except so far as the Australian four-class system prevailed, which, however, did not prevent first-cousin marriage. Morgan, *op. cit.*, pp. 425, 503.

⁵ Franz Boas, "The Central Eskimo," BAE, 6th Ann. Rept. (1888), p. 579.

⁶ James Teit, *The Thompson Indians of British Columbia*, AMNH, Memoirs, I (1900), 325.

⁷ H. J. Spinden, *The Nez Percé Indians*, AAA, Memoirs, II, Part III (1908), 250.

⁸ This conception of Morgan's will, however, prove to be erroneous.

sum up our conclusion to the effect that in North America exogamous kin groups, instead of being universal, were absent from a great many tribes, and that these for the most part possessed a less complex culture than those who had this institution.

Let us now turn to our second problem. Has the history of the one-sided exogamous group in North America been the origin and partial persistence of the archaic clan and its partial transformation into a gens? In other words, have all the gentes found among our Indians been preceded by a clan system? It will be best to consider first the mechanism by which Morgan conceives the change to have occurred and then the question of fact involved.

Roughly speaking, we find the gentile (as opposed to the clan) system among the Central Algonkian, Blackfoot, and Southern Siouan Indians. For the last-mentioned tribes Morgan suggests, as already noted, that the hypothetical change occurred under American and missionary influences. This suggestion, however, is anything but convincing. We know of a number of instances where civilization has introduced novel social arrangements among Indian tribes, but of none where it has produced Morgan's hypothetical development. Among the Crow, for example, the government has introduced a patrilineal rule of property inheritance, but the native rule of maternal descent continues to hold for clan names and affiliations. The Iroquois have adopted the system of passing on surnames given by whites from father to son, but the ancient matrilineal system remains in full force. The Crow have probably been subject to white influence for as long a period as the Southern Siouan tribes, and the Iroquois doubtless for a much longer period. It seems highly improbable that within the short period of something like a century contact with civilization should have caused a considerable number of tribes not merely to adopt the white way of reckoning descent in matters that would be of moment in their dealings with whites, but to be so thoroughly imbued with the point of view of the whites as to adopt the alien mode of tracing lineage in all parts of their social system. The Mandan, of whom not a dozen full-blood members survive at the time of writing, still reckon tribal affiliation according to the matrilineal scheme; children of Mandan mothers

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and Hidatsa fathers are Mandan; children of Hidatsa mothers are Hidatsa. Morgan's suggestion as to the cause of change of descent among Siouan tribes may therefore be dismissed as unsatisfactory.

With reference to the Algonkian Shawnee, Morgan makes a suggestion more in accord with his general scheme of development (p. 169). Instead of ascribing the change of descent to civilized influence, he is here inclined to assign an internal cause—the wish to enable a son to succeed his father as chief, and to enable children to inherit property from their father. But, repeating in essence the foregoing remarks, we must insist that both these questions—descent of office and descent of property—do not necessarily affect the fundamental matter of reckoning lineage. The Crow illustration cited above fits in here also, for it is manifestly a matter of indifference whether the rule of inheritance is changed from alien or indigenous causes. The question is whether a change in the rules of property inheritance from the maternal to the paternal line is itself a cause of changing clan affiliations into gentile affiliations; and there seems to be no evidence for this alleged causal connection.

This does not answer the question of fact whether, regardless of what causes may have operated, the gens is a development from the clan. Morgan's proof consists essentially in pointing out that while certain tribes have a gentile system other members of the same stock have clans. This is of course a two-edged argument that may with equal force be used to prove that clans developed from gentes. From the fact that Mandan, Hidatsa, and Crow reckon descent in the female line, Morgan argues that the Ponka, Omaha, Iowa, and Kaw formerly reckoned descent in the same way (pp. 155 ff.), all these tribes speaking Siouan languages. So, from the occurrence of female descent among the Delaware, Morgan infers "its ancient universality in this form in the Algonkian tribes" (p. 172). To be sure, this conclusion is supported by some additional data. The Delaware are declared to be "recognized by all Algonkian tribes as one of the oldest of their lineage," though it is safe to say that many Algonkian tribes were blissfully ignorant of the very existence of the Delaware in Morgan's time. Morgan furnishes better evidence in citing cases

of several Algonkian tribes with male descent where nevertheless the chief's office was passed, not from father to son, but from maternal uncle to sister's son (pp. 166, 170). However, these cases are very few, have not been corroborated by later inquiry, and admit of other explanations. For example, there may be special rules for the inheritance of certain offices distinct from those which otherwise hold. The coexistence of different rules of descent for different social groups is well established in various primitive tribes. Thus, in Uganda descent of clan membership was patrilineal for all except princes of royal blood, who were always reckoned of kin with their mother. Considering that even with the most favorable interpretation of the cases cited by Morgan we are still confronted with a considerable number of tribes with paternal descent and no trace of any other system, we must conclude that Morgan has not established his scheme of development inductively but deduced it from his a priori postulate of unknowable fatherhood in archaic times.

This brings us face to face with a most important theoretical problem. We have indeed shown that Morgan has not proved his case from the North American data; but he may nevertheless be right if others have established the general law that matrilineal descent precedes paternal descent. Extending our inquiry beyond the American data, we must admit that until recently most sociologists and anthropologists deduced this sequence from such postulates as the uncertainty of fatherhood among primitive conditions. Tylor's point of view was doubtless in large measure determined by such considerations, but he supports it on a more solid basis of fact than is usually the case, and accordingly it will be best to consider his reasoning in some detail. Advancing what he himself characterizes as a geological argument, he holds that

. . . the institutions of man are as distinctly stratified as the earth on which he lives. They succeed each other in series substantially uniform over the globe, independent of what seem the comparatively superficial differences of race and language, but shaped by similar human nature acting through successively changed conditions in savage, barbaric, and civilized life.

Tylor groups primitive tribes under three headings, corresponding to successive cultural strata: those with a maternal sys-

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tem of descent, those in which both maternal and paternal rules of descent coexist, and those with a purely paternal descent. He then examines, with reference to their occurrence in these strata, certain social customs—notably the remarriage of widows and the “couvade.” His treatment of the latter case will suffice to illustrate the method of reasoning followed. The couvade is the practice (found most conspicuously in some parts of South America) by which “the father, on the birth of his child, makes a ceremonial pretense of being the mother, being nursed and taken care of, and performing other rites, such as fasting and abstaining from certain kinds of food or occupation, lest the new-born should suffer thereby.” Tylor finds not a single instance of this strange usage among purely maternal peoples. In the maternal-paternal condition there are not less than twenty cases, while in the paternal the number dwindles to eight. From this Tylor infers that the purely maternal stage is the “earliest” because there is no survival of the couvade from other stages as there is in paternal society. Z

Just as the forms of life, and even the actual fossils of the Carboniferous formation, may be traced on into the Permian, but Permian types and fossils are absent from the Carboniferous strata formed before they came into existence, so here widow-inheritance and couvade, which, if the maternal system had been later than the paternal, would have lasted on into it, prove by their absence the priority of the maternal.⁹

In support of Tylor’s theory, that matrilineal institutions precede patrilineal descent, concrete evidence of all kinds has been adduced. Among the most recent writers, Rivers has expressed the conviction that this sequence holds for Oceania.¹⁰ On the other side, American ethnologists have appealed to the case of the Kwakiutl of British Columbia, where there is assumed to have taken place a change in the contrary direction. According to Professor Boas, the Kwakiutl, like the tribes of Oregon, Washington, and southern Vancouver Island, once lived in village com-

⁹ E. B. Tylor, “On a Method of Investigating the Development of Institutions, Applied to Laws of Marriage and Descent,” *RAI, Journal*, XVIII (1889), 245–269.

¹⁰ W. H. R. Rivers, “Survival in Sociology,” *Sociological Review*, VI (1913), 293–305.

munities with paternal descent. Owing to the influence of the more northern Pacific tribes, whose system is matrilineal, the Kwakiutl grafted the northern principle of descent on that of the south, with the result that certain privileges are inherited in the paternal line and a much larger number are obtained by marriage through an intricate method that insures maternal descent.¹¹ But although the Kwakiutl facts are very interesting, it is highly doubtful whether they have the theoretical significance ascribed to them. It is, in the first place, worth noting that they represent, in Tylor's terminology, not a maternal but a maternal-paternal stage. At best, therefore, they yield evidence of change from a purely paternal to a *mixed* condition. Secondly, maternal descent, so far as it prevails, seems to be restricted to the inheritance of property, while the reckoning of a child's affiliation seems to be indeterminate, as we have been more recently informed by Boas that a child is reckoned as belonging to both his father's and his mother's family. Thirdly, it is a matter of grave doubt whether the Kwakiutl units of which maternal-paternal descent may be predicated correspond to the type of unit to which Morgan, at all events, applies the sequence advocated by himself and Tylor. For Morgan is speaking all the time of *exogamous* units, whether clans or gentes, and among the Kwakiutl there seems to be no definite rule of exogamy but only a preference for marriage out of the group, and even this is denied in a later statement.¹² Finally, the Kwakiutl conditions are so specialized that adherents of the Tylor-Morgan theory may well regard them as exceptional; and even if the change from paternal to maternal descent be admitted, it is possible to suppose a pristine stage of matrilineal reckoning preceding the patrilineal village communities.

For these reasons the Kwakiutl conditions do not seem to furnish a favorable test case. Nevertheless, they embody the principle that forms the most vital objection to the classical theory as to rules of descent. For the Kwakiutl have developed their system not solely through internal growth but through contact with

¹¹ Franz Boas, "The Social Organization and the Secret Societies of the Kwakiutl Indians," USNM, *Report for 1895*, pp. 334-335.

¹² *Idem*, "Tribes of the North Pacific Coast," *Annual Archaeological Report*, 1905 (Toronto, 1906), pp. 239-240; A. A. Goldenweiser, "Totemism, an Analytical Study," *Journal of American Folk-Lore*, XXIII (April-June, 1910), 187, 213.

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other tribes. The far-reaching influence of such connection with neighboring tribes generally has been realized to an increasing degree by modern anthropologists, and it obviously interferes with the doctrine of parallelism advanced by Tylor. For, granting that on account of the similarity of human nature, human institutions *tend* to succeed one another "in series substantially uniform over the globe," the borrowing of institutions would in an indefinite number of cases produce an abnormal sequence. We cannot even assert that where the observed sequence corresponds to the theory the result is due to uniform causes producing parallel evolution. Among the Carrier and Babine Indians there is matrilineal descent. As the majority of the Northern Athapascans, of whom these tribes form part, have a loose organization, it may be safely assumed that the Carrier and Babine once shared this sociological characteristic, provided we can indicate the conditions that in their case produced a change. We thus seem to have an illustration of the evolution of a clan system from the "earlier and less organized and regulated condition" postulated by both Morgan and Tylor. But the conditions that produced the change were not so general as the psychological constitution shared by humanity, but lay in the geographical contiguity of the Northwest Coast Indians, whose social organization was simply copied by the tribes in question. Accurate information as to the actual process of cultural development has largely shattered the belief once held in the necessity of parallel evolution among unrelated tribes. Many ethnologists now hold that historical processes are unique in character, that every phase of human history is so complicated by individual traits that no laws of historical development can be framed. This view has so deeply affected modern anthropology that even in quarters peculiarly liable to classical influence a far more cautious formulation is now in vogue. It is no longer contended that every gentile system has superseded a clan system, but merely that *if* the rule of descent changes at all, it changes from matrilineal to patrilineal descent. Thus, N. W. Thomas writes:

. . . whereas evidences of the passage from female to male reckoning may be observed, there is virtually none of a change in the opposite direction. In other words, where kinship is reckoned in the

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female line, there is no ground for supposing that it was ever hereditary in any other way. On the other hand, where kinship is reckoned in the male line, it is frequently not only legitimate but necessary to conclude that it has succeeded a system of female kinship. But this clearly does not mean that female descent has in *all* cases preceded the reckoning of kinship through males. Patrilineal descent may have been directly evolved without the intermediate stage of reckoning through females.¹³

And expressing a still more acceptable view, Cunow writes:

Die meisten der heutigen vaterrechtlichen Halbkulturvölker haben sicherlich einst, wie sich deutlich aus ihren Rechtsbräuchen und Verwandtschaftsbezeichnungen nachweisen lässt, das Mutterrecht gekannt; aber das besagt noch nicht, wie den Vertretern der zweiten von Müller-Lyer genannten Theorie eingeräumt werden muss, dass das Mutterrecht eine Institution ist, die sich bei *allen* Rassen und Völkern ohne Unterschied auf gewisser Entwicklungshöhe einstellt. Unter besonderen Umständen mag das Mutterrecht ganz gefehlt haben oder doch die Mutterrechtsperiode von relativ kurzer Zeitdauer gewesen sein.¹⁴

From this modern point of view there is thus no reason to suppose that the Blackfoot, Central Algonkian, and Southern Siouan tribes ever possessed a clan system preceding their present or recent gentile system. Their general cultural condition, whatever may be the value of such a comparison, does not show a higher stage than that of maternally organized tribes; of the latter, indeed, the Pueblo Indians are manifestly superior to any of the patrilineal tribes. Considering the modified form in which such sane students as Marett, Cunow, and Thomas now present the classical theory of father-right and mother-right, we may safely say that there is no reason why the patrilineal tribes of North America could not have developed their system directly from a loose organization without passing through the hypothetical intermediate stage. Summing up, therefore, our reply to the two questions set at the beginning, we may say:

1. It is as certain as anything can be from the nature of the case that the one-sided exogamous kin-group system, whether

¹³ N. W. Thomas, *Kinship Organization and Group Marriage in Australia* (Cambridge: University Press, 1906), p. 15. Cf. R. R. Marett, *Anthropology* (New York, 1912), p. 169.

¹⁴ Heinrich Cunow, in *Zur Urgeschichte der Ehe und Familie*, pp. 38-39.

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in the form of clans or gentes, was not universal among North American tribes.

2. It is entirely unproved that those Indian tribes possessing a gentile system previously had a clan system.

II

In the preceding discussion little has been said of what is really more important in Morgan's treatment than either the geographical distribution of one-sided kin groups or the relative priority of different forms of these groups—Morgan's notion of what the kin group really represents. There can be no doubt that in Morgan's mind it was primarily not only a marriage-regulating agency (pp. 74, 378), but *the* marriage-regulating agency, preceding a prohibition of marriage between blood-relatives generally. It was, moreover, the only original type of social unit he recognized in primitive society beyond a certain stage and before the development of political society (p. 63). Finally, it was everywhere the same (*ibid.*).

Every one of these points is open to criticism. I shall first endeavor to show that blood-relationship operated as a bar to marriage independently of the origin of the one-sided kin system.

If Morgan's view of the kin group were correct, tribes possessing this institution ought, in the first place, to consider marriage with the most distant cousin belonging to one's own group as incestuous as marriage with an own sister, for the exogamous rule according to hypothesis was a bar to brother-sister marriages only indirectly, inasmuch as brothers and sisters were included in the number of kinsfolk among whom marriage was tabooed. It is true that there are cases where union with a fellow-member of the same kin group is regarded as incest, even where there is no trace of blood-relationship between the mates. Among the Haida, for example, "so close was relationship held to be between persons of the same clan, that marriage within it was viewed by them almost as incest is by us."¹⁵ But in other cases there is no such feeling. The Crow and Hidatsa have preserved their matrilineal system to the present day, the former still considering

¹⁵ J. R. Swanton, *Contribution to the Ethnology of the Haida*, Publications of the Jesup North Pacific Expedition (New York, 1909), I, 62.

their clans exogamous. Yet transgression of the exogamous rule is a matter for ridicule not for punishment, is considered improper rather than criminal, and there is no evidence that any other point of view was ever taken by the natives.¹⁶ The same, according to Dr. Goldenweiser, applies to Morgan's own Iroquois: mild reproof, not abhorrence, was meted out to the offenders. In these tribes marriage with an unrelated clansman would not begin to compare in offensiveness with marriage to a clansman who was a near blood-relative.

It may of course be urged that the differentiation of clansman and blood-relative is recent, but the argument may be supplemented by another. Morgan's theory presupposes not only that all members of a kin group are equally barred from marriage, but also that marriage with members of other kin groups is permitted regardless of ties of consanguinity. This corollary is demonstrably false for the present time in a great number of cases, and with it falls the theory from which it is deduced. Among the Crow, marriage with a near relative on the father's side is as strictly tabooed as marriage with a near relative within the clan. The same applies to the maternally organized Creek,¹⁷ to the Navaho, Iroquois, and Zuñi.¹⁸ Among the Omaha, while a man must marry outside of his gens, he is also required to marry outside of his mother's gens.¹⁹ The same, to choose an example from an entirely different area, applies to the patrilineally organized Baganda of East Africa.²⁰ Among the Australians of Queensland blood-cousins are not allowed to marry though they belong to otherwise inter-marriageable groups.²¹

Against this twofold argument the only possible answer is that the kin-group affiliation was in each case primary; that the attitude toward marrying a related clansman, as compared with

¹⁶ R. H. Lowie, *The Social Life of the Crow Indians*, AMNH, Anthropological Papers, IX (1912), 188 f.

¹⁷ J. R. Swanton, "A Foreword on the Social Organizations of the Creek Indians," *American Anthropologist*, XIV (1912), 596.

¹⁸ *Idem*, "The Social Organization of the American Tribes," *American Anthropologist*, VII (1905), 667-668.

¹⁹ J. O. Dorsey, "Omaha Sociology," BAE, 3d Ann. Rept. (1884), p. 257.

²⁰ John Roscoe, *The Baganda* (London, 1911), p. 128.

²¹ W. E. Roth, *Ethnological Studies among the North-West-Central Queensland Aborigines* (Brisbane, 1897), p. 69.

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that toward marriage with an unrelated one, and the objection against marriage with related members of other clans developed *after* the feeling against kin intermarriage. Here it may be noted in the first place that absolutely no evidence exists for this view; it is pure dogma. On the other hand, there is good evidence for the reverse order of development. As noted above, it is precisely among those North American Indians possessing the simplest culture that ties of blood-relationship, and not of one-sided kinship, act as a bar to marriage. Examples could be drawn from among tribes of other continents. Thus, among the Kai of New Guinea, marriage between a brother and sister is forbidden, though nothing is said of any kin restriction. In the same area the Jabim prohibit unions between children of a brother and sister.²²

There is even more direct evidence. In several instances it appears that the exogamic character of the clan or gens is derived from the feeling that all fellow-clansmen or gentiles are related by blood. Among the Blackfoot,

when a proposal for marriage has been made, the relatives of the girl get together and have a talk, their first and chief concern being the question of blood relationship. Naturally, the band [gentile] affiliations of the contracting parties cannot be taken as a criterion, since both may have very near relatives in several bands and cousins of the first degree are ineligible. Should the contracting parties belong to the same band but be otherwise eligible, the marriage would be confirmed, though with some reluctance, because there is always a suspicion that some close blood relationship may have been overlooked. Thus, while this attitude is not quite consistent, it implies that the fundamental bar to marriage is relation by blood, or true descent, and that common membership in a band [gens] is socially undesirable rather than prohibitive. . . . In any event, the attitude of the Blackfoot themselves seems to imply that the band [gentile] system came into existence after the present marriage customs and adapted itself to them rather than they to it.²³

A still more striking case is furnished by the Todas of Southern India. The Todas, according to Rivers, have a general term, *pūliol*,

²² Keysser and Zahn, in Richard Neuhauss, *Deutsch Neu-Guinea* (Berlin, 1911), III, 89, 299.

²³ Clark Wissler, *The Social Life of the Blackfoot Indians*, AMNH, Anthropological Papers, VII (1911), 19-20; also in *Current Anthropological Literature*, I (1912), 15-16.

for those relatives whose intermarriage is prohibited, including some that belong to the same gens and some who do not. A man casting about for a mate classified women as either *pūliol* or not, "and it seemed to me in several cases as if it came almost as a new idea to some of the Todas that his *pūliol* included all the people of his own clan [gens]." A Toda

has not two kinds of prohibited affinity, one depending on clan [gentile] relations, and another on relations of blood-kinship, but he has only one kind of prohibited affinity, to which he gives the general term *pūliol*, including certain kin through the father and certain kin through the mother, and there is no evidence that he considers the bond of kinship in one case as different from the other as regards restriction on marriage.

The fact that the Toda includes all those kin whom he may not marry under one general term, and that the kin in question include members both of his own and other clans [gentes], goes to show that the Todas recognize the blood-kinship as the restrictive agency rather than the bond produced by membership of the same clan [gens].²⁴

That is to say, among both Blackfoot and Todas such gentile exogamy as occurs is not a primary but a derivative phenomenon—is merely a function of the primary phenomenon, to wit, of restriction on the basis of blood-relationship.

To sum up. Restrictions of marriage based on blood-relationship apart from one-sided kinship coexist with kin-group exogamy. Restrictions of marriage due to blood-relationship exist where kin exogamy does not occur, that is to say, in North America at least, among the tribes with the most primitive culture. Finally, there is positive evidence that in some cases kin exogamy is a corollary of restrictions due to blood-relationship. The conclusion seems warranted that clan or gentile exogamy is a phenomenon superimposed on prohibitions of incest in our sense.

Let us now turn to the question whether the clan or gens is really the prototype of all social units in North American Indian society. Morgan found that among the Seneca-Iroquois the eight clans of the tribes were grouped in two divisions of four each. These larger divisions or "phratries," he argued, were nothing but overgrown clans that had become subdivided, the subdivisions

²⁴ W. H. R. Rivers, *The Todas* (London, 1906), pp. 509–510.

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preserving a consciousness of their kinship.²⁵ As association of clans occurs without a dual grouping I propose to call any such association a "phratry," and will call "moiety" one of two complementary divisions of a tribe, regardless of its relations to whatever lesser divisions may coexist with the moieties. In my terminology the question then arises: Did the Seneca moieties develop from the Seneca clans by a process of subdivision?

In order to solve the problem, let us consider the facts as presented by Morgan and Dr. Goldenweiser. The Bear, Wolf, Beaver, and Turtle clans form one moiety; the Deer, Snipe, Heron, and Hawk the complementary moiety. While the clans bear animal names, the moieties are nameless. At the present, and indeed this even applied to Morgan's time, the moieties are not exogamous, but there is evidence that they were exogamous long ago; the clans remain exogamous even today. Each clan has a set of individual names distinctive of its members, and there is a rule that none of these names shall be borne by more than one person at a time. The moieties do not possess distinctive sets of names, but on their part exercise certain functions not shared by the clans. At a ball game the division of players and of their supporters in betting was by moieties; at tribal councils the chiefs of the two moieties sat on opposite sides, and voting on such a subject as the confirmation of a new chief was by moieties; at a funeral the moiety complementary to that of the deceased conducted the ceremonies; finally, there were obscure religious functions connected with the phratries.

These being the essential facts, what do we learn from them respecting the segmentation of two primary clans into eight? Before answering this question, let us try to determine whether we have any evidence for the segmentation of social units. Such evidence undoubtedly exists. Among the Onondaga-Iroquois, there is not only a Big Snipe but also a Little Snipe clan. If these were primarily distinct social units, we should expect to find that their sets of individual names were distinct, this being a characteristic of the clans of all the confederated Iroquois tribes. But the clans in question share the same set of names, whence their essential unity may safely be inferred. In other tribes additional

²⁵ Morgan, *op. cit.*, pp. 88, 89, 91, 99.

examples of segmentation could doubtless be adduced. Where different clans are named for animals of the same genus but for different species of that genus, there is at least some presumptive evidence for segmentation (though never more), and where the moiety bears the name of a predominant clan or gens within that moiety the same applies, though here the actual demonstrative value of the evidence is even less.

However, in the case of the Iroquois it is not at all clear why the moiety and the clan should be supposed to be genetically related. Either the moiety was not formerly exogamous—then there is no functional similarity between moiety and clan at all; or, as according to Morgan and Dr. Goldenweiser it is reasonably certain, the moiety was once exogamous. Then, also we are not justified in saying that *both* units were exogamous. For, at the time the moiety was exogamous, the clan was by logical necessity exogamous as a part of the exogamous moiety, while its distinctively *clan* characteristics may have had nothing to do with exogamy.²⁶ The fact that when the exogamous rule of the moiety broke down it was limited to the clan proves nothing as to the unity of the two organizations. For in Australia, as Cunow shows, the marriage-regulating functions of the classes have in some instances been transferred to the genetically quite different totem kin units. It is true that an indefinite number of excuses can be given why proof of connection between the Iroquois moiety and clan should be lacking. Names of social units have been known to disappear; social units have been known to assume new functions; if the moiety developed according to Morgan's scheme, evidence of the exogamic character of the new clans must have disappeared. All such explanations remind one of the evolutionist's plea as to imperfections of the paleontological record. Such a plea is admissible where there is extraneous positive evidence, but does not fill the place of lacking evidence. In the case of evolution there is fortunately independent evidence; in our Iroquois case there is not. Hence, the unity of the Iroquois clan and moiety remains unproved, though not disproved.

²⁶ This argument has been used by Dr. Goldenweiser with relation to Australian conditions.

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The value of the foregoing discussion lies in several directions. For one thing it changes our view of the essence of the units commonly called clans and gentes. Whenever such units form part of larger exogamous units, we can no longer assume dogmatically that they, too, are at bottom exogamous. This, nevertheless, remains a possibility, and on that assumption we must change our conception of the moiety. The moiety, instead of being a subdivided exogamous clan, may be an association of exogamous clans constituting a unit of novel character even if it assumes the exogamous character once distinctive of the separate clans. And this again opens our eyes to all kinds of possibilities. We need no longer tug at all the facts of social organization in a vain effort to thrust them into the strait-jacket of "exogamy." We begin to suspect that various types of social units may peaceably coexist in the same tribe, some regulating marriage, some, other social activities; nay, some regulating marriage in one sense, others, in a different sense. In short, instead of the dull uniformity of the theorists, we may have all the motley variety of real life with its profusion of individual differences. To justify this pluralistic view, let us turn to some facts.

The Fox and Kickapoo are divided into exogamic gentes bearing animal names. So far these units, except for the rule of descent, correspond to the Iroquois clans. But in addition to their gentes both Fox and Kickapoo have a division into moieties that is utterly different from the Iroquois scheme. The Kickapoo child enters a moiety only after receiving a name.

The name comes from the father's name, unless the right of naming the child is handed over to the mother by the father. If the father is *uskaca*, then the offspring will be *uskaca*. If the mother is *kickō'a* and she has the right of giving the name, then the child is *kickō'a*. Again, the child can become a *kickō'a* if he is given to a grandmother, grandfather, sister's son, or a sister's daughter; the child gets his name from the one in whose hands he falls, and if the namer is a *kickō'a* the child will be a *kickō'a*.

Among the Fox the father usually, but not always, determines which division a child shall enter. "If he is a Tō'kân, it is likely his children will be the same. Often the firstborn is the same as

the father, and the next child is the other. No distinction is made on account of sex." ²⁷

From an earlier account of the closely related Sauk it appears that consecutive children of the same father were placed into different moieties, the oldest into his father's, the next oldest into the complementary moiety, and so forth.²⁸ The Fox and Kickapoo moieties are distinguished by the use of different paint for personal decoration. Their sole function has been limited to that of divisions in athletic games, but according to Dr. Michelson, the Fox moieties had more serious (in part, ceremonial) duties. These moieties differ markedly from those of the Iroquois, not so much because of a difference in function, as in essential constitution. Functions may be assumed and lost. The moieties of the Fox might be supposed to have lost some of the characteristics found among the Iroquois, or the Iroquois moieties might be supposed to have originated as divisions similar to the Fox moieties and to have assumed additional duties. But the difference is more fundamental. While among the Iroquois *all* the members of several gentes are united in one moiety, this is contrary to the Fox scheme, where members of the same gens belong to opposite moieties, while either moiety probably joins together members of all the gentes. The problem that, however solved or however insoluble, naturally arises from the Iroquois facts, viz., whether the moiety is a subdivided clan or an association of originally distinct clans, does not arise at all in connection with the Sauk and Fox. It would not even arise if the rule of descent were definitely patrilineal for the moiety as it is for the gens. For there is nothing to show that a man of gens *a* must belong to moiety A; and so long as there is no such definite correlation, the children of a given man, *Aa*, will indeed be also *Aa*, but will be joined for athletic purposes (or what not) by the children of *An*, who are also *An*. Consider Germans divided into Catholics (A) and Protes-

²⁷ William Jones, "Notes on the Fox Indians," *Journal of American Folk-Lore*, XXIV (1911), 216, 220; *idem*, "Kickapoo Ethnological Notes," *American Anthropologist*, XV (1913), 335. According to Dr. Michelson the firstborn child belongs to the moiety complementary to its father's, the second to its father's moiety, and so forth.

²⁸ Thomas L. McKenney and James Hall, *History of the Indian Tribes of North America* (Edinburgh, 1933), I, 117.

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tants (B). From the point of view of marriage the German Catholics form as definite an endogamous unit as the Fox gens forms an exogamous unit. But Germans are also divided into socio-economic groups, *a* to *n*, within which membership is in a measure hereditary, and when we are told that a German is of the landed gentry, a capitalist, a proletarian, etc., we do not know whether he is a Catholic or a Protestant, nor will it occur to us to ask, whether the religious division grew out of the economic division or vice versa.

An even more instructive case is furnished by the Yuchi, formerly resident in Georgia and Alabama but now settled in Oklahoma. Like the Fox, the Yuchi are divided into exogamous groups, though with them descent in these groups is matrilineal. Like the Fox again, the Yuchi are divided into moieties, but with the important difference that membership descends in the paternal line. Here again each moiety will thus embrace members of all possible clans and each clan will have members of both moieties. The functions of the Yuchi moieties are of great importance. Not only does the division during ball games follow moiety lines, but in every phase of ceremonial, military, and political life the moiety division appears more important than the clan division. The Chief moiety is associated with peace, the Warrior moiety with war. From among the Chiefs were chosen the highest public officials, while the Warriors took the initiative in setting out against the enemy. At the tribal ceremony the Chiefs cared for the medicine plants, while the Warriors presided over dances and games. As if to emphasize the distinctness of the moiety from the clan, there is a tendency—though not consistently carried out—for Chiefs to marry their daughters to other Chiefs rather than to Warriors.²⁹ In considering the social organization of the Yuchi, it would be monstrous one-sidedness to disregard the important dual division and emphasize only the exogamous clan unit.

But the social relations of individuals may be definitely determined, even without a definite grouping under the same group name. Among the Hidatsa there were formerly seven exogamous clans, four of which were grouped in one moiety, and the re-

²⁹ Frank G. Speck, *Ethnology of the Yuchi Indians*, University of Pennsylvania, Anthropological Publications, University Museum, I (1909), esp. pp. 70-78.

mainder in the complementary moiety. Membership in clan and moiety depended on one's mother's affiliations. But a Hidatsa's social activities were only in part dependent on his membership in the two units mentioned; in very large measure they depended on his father's membership. The ceremonial life of an individual was determined by his father's: he was obliged to purchase certain sacred objects from his own father, and with them membership in an esoteric ceremonial fraternity. Moreover, all the children of fathers belonging to the same clan were united in a group of "joking relatives" whose privilege it was not only to play practical jokes on any of the members of the group, but also publicly to upbraid any member offending against tribal custom. Of the Hidatsa, also, we may therefore say that their social organization is very imperfectly described by an account of the exogamous clan system: at any particular period the group of "joking relatives" and of the patrilineal esoteric brotherhood loomed as equally important social factors with one's own clan.

These few illustrations of different types of social units by no means exhaust the number found in North America. From among the most interesting ones I may add the castes of the Northwest coast, where tribes are divided into Chiefs, Noblemen, Commoners, and Slaves; and loose associations of clans into phratries with apparently few or no distinctive traits, which occur among the Crow. Instead of finding North American society built upon a single basis, the clan or gens (for in Morgan's scheme, I repeat, the phratry or moiety is only a segmented clan or gens), we have met the following varieties of social unit:

1. The exogamous clan or gens (Crow, Fox).
2. The clan or gens of indeterminate character as to exogamy, because it forms part of a larger exogamous unit (Iroquois).
3. The exogamous moiety composed of several clans or gentes (Iroquois).
4. The non-exogamous moiety composed of several clans or gentes (Hidatsa).
5. The non-exogamous moiety organized without relation to clans or gentes (Yuchi, Fox).
6. Phratries of indeterminate character (Crow, Kansas).
7. Castes (Northwest coast).

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8. Ceremonial bodies in which membership is determined by descent (Hidatsa).

To these, on the basis of former considerations, we must add the family in more or less our sense of the term, for we found that both clan or gentile systems exist and where they do not exist there are social relations due to ties of blood-relationship independently of such systems.

To regard the exogamous one-sided kinship group as the sole basis of social organization in North America is thus an unjustifiable piece of anthropological folklore.

Finally, we must take up the question, whether the kinship group is everywhere fundamentally the same. To a certain extent this has already been answered. For if, on the one hand, clans are exogamous as such, like those of the Crow, while on the other hand they are only derivatively exogamous, as parts of other units that *are* exogamous in their own right, then, of course, whatever fundamental unity exists, exists, from Morgan's point of view, between the exogamous divisions and not between the divisions called clans. And as a definite unit must serve *some* purpose, the non-exogamous clan must have had some other characteristic that puts it in a different class from the exogamous clan. This being so, we must repudiate as dogmatic the assumption that wherever clan exogamy is found with other features the exogamous feature is the historically earliest trait with which the other traits afterward become associated. It is entirely possible that the course of development may in some cases have been as follows: There may have been a ceremonial group³⁰ at the beginning, with a rule of descent like that followed in the Hidatsa esoteric groups, viz., the rule that children buy their own father's medicines and membership. The ceremonial group will thus come to consist of a number of patrilineal families. We need only the additional step that marriage shall be tabooed among fellow-members—a step that has been taken in ceremonial associations of a different type within the same area—to have a typical gens, with exogamy as the final instead of the fundamental feature of organization.

³⁰ Or a group constituted for some other purpose. I adopt an example patterned as closely as possible on actual facts.

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In the light of these considerations we shall not base any theory as to the fundamental unity, either historically or psychologically, between two given clan systems on the existence of exogamy in both, but shall take into account all the geographical, historical, and psychological factors that would be considered in any other ethnological comparison. Let us compare, for example, the clan concept of the Iroquois with that of the Crow. The Iroquois clan bears an animal name; has a distinctive set of personal names; and was once derivatively exogamous. The Crow clan is not named for an animal, but bears a nickname, such as "Sore-lip," "Tied-in-a-Knot," "Bad War Honors"; it has no set of personal names; and it is primarily exogamous, for it does not form part of a larger *exogamous* unit. This parallel becomes really significant when we view both systems in their geographic setting. Clans and gentes bearing names directly or indirectly referring to animals are found very widely distributed among the tribes east of the Mississippi and among the Southern Siouan tribes. The idea of nicknaming clans, gentes, or local bands is also definitely localized among the tribes of the Northwestern Plains—Assiniboiné, Dakota, Blackfoot, Crow. Practically all the tribes with animal-named divisions have associated with these divisions sets of distinctive personal names; among the Northwestern Plains tribes children are named either in commemoration of some exploit of a distinguished tribesman or according to a supernatural revelation, a method precluding clan sets of personal names. Finally, there is fair evidence of exogamy being a secondary clan feature among a number of Eastern Woodland and Southern Siouan tribes. The Iroquois, Winnebago, and Kansas are positively stated to have had exogamous *moieties*; for the Osage the same condition seems to have held formerly;³¹ for the Omaha there is at least some indication of pristine exogamy in the moiety. While the evidence on this point is far from convincing, a suspicion remains that in the area under discussion exogamy may have been primarily associated with the dual division. But in the Northwestern Plains area there is no reason for assuming that exogamy was anything but a phenomenon characteristic of the clan or gens, for the simple reason that the moiety does not

³¹ For this information I am indebted to Miss Gerda Sebbelov.

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occur among the Crow and Blackfoot, and the phratries of the Crow are historically a later development than the exogamous clan. The clans of the Iroquois and Crow are therefore not only radically dissimilar, but it appears that they represent two types of social unit distinctive of certain definite geographical areas. If kinship groups are not fundamentally alike even within the same continent, they will, a fortiori, not be fundamentally alike in different continents, as Dr. Goldenweiser has shown by a comparison of Australian and Northwest American kinship groups.³² The theory that clans or gentes conform to a single basic concept thus breaks down utterly.

Our critique of Morgan has thus established the following conclusions:

1. Kinship groups tracing descent unilaterally are not found universally among primitive tribes.
2. It is not proved that the North American gentes developed out of clans.
3. Restrictions of marriage are not primarily determined by unilateral kinship groups, for they exist, on the basis of blood-relationship, where no such groups exist, and coexist where such groups do exist; kinship groups being absent precisely among the tribes of lowest culture (in North America).
4. The exogamous kinship group did not form the sole foundation of the social fabric among primitive tribes, where quite different units, such as the moiety, caste, etc., occur, often coexisting with the clan or gens.
5. The kinship group is a phenomenon of variable significance.

Every destructive criticism of a view sanctioned by tradition leaves its adherents with a sense of loss. This feeling is of course an illusion, for there is no real loss when opinions are abandoned that are demonstrably false. But does modern ethnology, to use a hackneyed phrase, merely tear down without building up? To anyone acquainted even with the rudiments of psychology this question must seem very naïve. Synthesis is the most fundamental characteristic of consciousness: all the elementary operations of the human mind, such as the association of ideas, are described by

³² Goldenweiser, *op. cit.*, pp. 179–293.

psychologists, such as Höffding and Cornelius, as *synthetic* processes. Under these circumstances the fear that modern critical science will compile a chaotic mass of data by failing to correlate the isolated facts it ascertains is absurd. The kind of synthesis that some people dream of seems to be the method of insisting that things are alike which one knows to be different, and it is this kind of synthetic sleight-of-hand performance with which modern science will have nothing to do. Let us once more turn to the facts to see in what manner legitimate synthesis proceeds.

We find that the Crow are divided into clans of a certain type—an isolated fact. When we find subsequently that the Hidatsa have exactly the same type of clan we *spontaneously* bring this fact under the same heading as the first—we synthesize, in other words, our two findings. This spontaneous activity, owing to the very nature of human consciousness, must be supplemented by a systematic attempt at all conceivable correlations of fact. How did the Crow-Hidatsa clan originate? Is it a trait of human psychology to evolve just such a clan in every part of the globe? Or is such a development due to a peculiarity of the Crow and Hidatsa? Or has it been borrowed from certain other tribes, and if so, from what tribes? The formulation of every such problem is the synthesis of certain facts that were at first isolated, unsynthesized. But the synthesis consists solely in the formulation itself. If we find that a suggested correlation does not hold as a matter of fact, our method of procedure has nevertheless been synthetic, quite independently of the result. When Morgan assumes without question that the Iroquois clan and moiety are at bottom alike, he is not more synthetic but less synthetic than we who question the self-evident character of his assumption and thus suggest a new coördination of the facts.

The actual course of scientific progress is more enlightening on the subject of scientific method than the psychologically intelligible, but logically often indefensible, desire for simplicity. We must indeed seek the simplest, most economical representation of the facts, but the emphasis must be as strong on “facts” as on “economical” if we are to avoid producing merely a pleasing myth instead of a summary of reality. What does it mean when Faraday tells us that he crushed dozens of hypotheses in the

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silence of his laboratory? It means that, thoroughly saturated with the facts of his science, Faraday attempted all manner of correlations of phenomena, many of which could never have suggested themselves to one unacquainted with the same range of facts, and that, by a process of selection, those correlations suggested in his thinking to which there corresponded a real correlation in nature remained as his permanent contribution to science.

If anthropology is to be regarded as a science, it must conform to the logical methods of the exact sciences. Spontaneous synthesis of anthropological facts will be supplemented by systematic suggestion, verification, and elimination of all conceivable coördinations. In this work there will naturally be differences among anthropologists due to individual differences in knowledge of anthropological fact, individual differences in degree of synthetic faculty and of critical judgment. But can anthropology aspire to the exactness of sciences like physics and chemistry? As regards logical method, at least, I firmly believe it can; if I did not, I should regard it as a harmless mode of amusement or as a branch of *belles-lettres* rather than as a branch of science.

One or two illustrations must suffice. Among several of the Plains Indian tribes there are graded clubs, the members of any one of which are of the same age and buy their place in the society. We have here, then, two factors on either of which membership may primarily depend—purchase and age. The question is, which is the fundamental correlation, membership and purchase, or membership and age? Let us make an experiment in thought. Provided membership is a function of age, then if we vary the age beyond a certain limit membership must cease. Provided membership is a function of purchase, it will cease only when sold. Fortunately, our experiment in thought has been performed for us in reality. Among the Hidatsa and Mandan, individuals have preserved their membership in clubs regardless of advancing age, because they had had no opportunity to sell their membership; in this way some claim simultaneous affiliation with more than one organization, which of course would be impossible if these clubs were primarily age societies. Hence, membership is basically a function of purchase. Many theories of physicists as to

the constitution of matter rest, I fear, on a less solid basis of fact and logic.

To take another case. Many primitive tribes use the same term in addressing relatives very differently related according to our notions of kinship. For example, the Crow call a father, a father's brother, the husband of a mother's sister or of a father's sister by the same term. They also use a single word in addressing a mother, a mother's sister, a father's sister, and the wife of a father's brother. From this we might infer that persons of the same generation and sex are addressed by the same kinship term. But while this conclusion is in large measure true, it does not state the whole truth. For a Crow will also address any father's clansman, no matter how young, as "father." Similarly, he will address any female of his father's clan, no matter how young, as his "mother." Now, as among the Crow descent is traced in the female line, any man, his sister, and her daughters and sons, her daughter's daughter, and all female descendants indefinitely must belong to the same clan. Hence, if I am a Crow, I ought to call my father's sister's daughter and all her female descendants "mother," and my father's sister's son "father," because all of them belong to my father's clan. This is actually the case, both among the Crow and the related Hidatsa. But how can we make sure that they are so called *because* of their clan? Obviously we must proceed again by keeping other possible causes constant and determining what happens when we eliminate the one cause under discussion. We must, therefore, find two relationships differing only as to clan affiliation. Now, while my father's sister's daughter and son belong to the same clan (which is also my father's), the daughter of my father's sister's son and the daughter of my father's sister's daughter will belong to different clans, since the former must follow her mother's clan. Hence we have here two relatives identical in sex and generation, but differing in clan. We have found that a Crow will call his father's sister's daughter's daughter "mother." Does he call his father's sister's son's daughter by the same term? He does not, calling her instead his "sister." In other words, when the clan factor is eliminated, a different factor becomes potent—the generation factor. For it seems clear that I use the term "sister" because the father of my "sister" is my

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“father” and because children of the same “father” are brothers and sisters.

There are no bounds to the synthetic coördination of ethnological facts along the lines here indicated. That, however, ethnological facts should admit of such simple wholesale summing up as certain (not all) groups of physical facts is an unreasonable demand. If ethnological laws of development exist, their discovery will doubtless be a great achievement. But we must be on our guard against “fake” laws that do not result from a synthesis of the *facts* but from an artificial simplification by selection of those facts that fall in with the investigator’s fancies. It would be a great simplification of much physical calculation if bodies fell with an acceleration of 10 meters per second; but they perversely persist in falling with an acceleration of 9.81. So it would be charming if all tribes passed first through a loose, then through a clan, and finally through a gentile form of organization; unfortunately there is no evidence that many of them do. It has been said in Newton’s praise that he did not attempt to astonish himself by his clever ideas about nature, but sought to know what nature was really like. In looking over ethnological literature we are frequently tempted to ask whether the writer’s object is not solely to amaze himself and others by his own cleverness. But ethnology is rapidly coming of age, and we are learning to synthesize after the manner of Newton, in the expectation of raising, not a structure of new anthropological folklore, but a new anthropological science.

2

Exogamy and the Classificatory Systems of Relationship

SOME CONNECTION BETWEEN EXOGAMY AND PRIMITIVE relationship terminologies has been recognized for a long time. Morgan noted that among the Iroquois all clan members were brothers and sisters as if children of the same mother.¹ And though in his theoretical treatment of the subject he does not derive the classificatory system as a whole from the exogamous principle, he does attribute the change from the older Malayan to the later and more common Turanian form of the system to punaluan marriage as a predecessor of the institution of exogamy and to exogamy itself.² Tylor, to my knowledge, was the first to view exogamy and the classificatory system as but "two sides of one institution."³ More recently both Frazer⁴ and Rivers⁵ discovered

A paper read before the American Anthropological Association at Philadelphia. Printed in *American Anthropologist*, XVII (April-June, 1915), 223-239.

¹ L. H. Morgan, *League of the Ho-dé-no-sau-nee or Iroquois* (New York, 1922), Book I, chap. 4.

² *Idem*, *Ancient Society* (New York, 1877), Part III, chaps. 1-3.

³ E. B. Tylor, "On a Method of Investigating the Development of Institutions, Applied to Laws of Marriage and Descent," *RAI, Journal*, XVIII (1889), 245-269, esp. pp. 261 ff.

⁴ J. G. Frazer, *Totemism and Exogamy* (London, 1910), IV, 114.

⁵ W. H. R. Rivers, "On the Origin of the Classificatory System of Relationships"

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the origin of the classificatory system in "a social structure which has the exogamous social group as its essential unit," both conceiving this group as an exogamous moiety, which indeed had already figured prominently in Tylor's essay.

It will be best to put this theory in somewhat more concrete form. Among the Iroquois, Morgan noted that a single term was applied to the maternal grandmother and her sisters; to the mother and her sisters; to the father and his brothers; and so forth. On the other hand, distinct terms were applied to the father's and to the mother's brother, to the father's and to the mother's sister.⁶ All these facts are readily formulated by deriving the classification from the exogamous groups extant among the Iroquois: those relatives distinguished in our own nomenclature and not distinguished in that of the Iroquois are members of the same exogamous division, while those not distinguished by us and separated by the Iroquois are necessarily members of different divisions. From this point of view the objection otherwise plausibly urged against denying the name "classificatory" to our own system since it, also, ranges certain relatives in *classes* becomes impossible. It is no longer a question, whether our terms "uncle," "aunt," or "cousin" are "classificatory" in a purely etymological sense of the term; nor whether the classificatory principle is quantitatively more important in certain primitive systems than in our own.⁷ The point at issue is the *basis* of the classification, and having regard to this there obviously exists a real difference between a system that classifies, say, cousins from both the father's and the mother's side under a common term and a system that rigorously divides relatives of the paternal and the maternal line on the ground of their different clan or gentile affiliations. Thus, the Tylor-Rivers theory, on the one hand, briefly summarizes and makes intelligible certain modes of classification operative in many primitive systems that otherwise might seem purely capricious; and, moreover, it furnishes at last a

in *Anthropological Essays Presented to Edward Burnett Tylor* (Oxford, 1907), pp. 309-323; *idem*, *Kinship and Social Organisation* (London, 1914), pp. 70 ff. The latter is an astonishingly stimulating contribution to the whole subject of kinship nomenclature.

⁶ Morgan, *loc. cit.*

⁷ Rivers, *Kinship and Social Organisation*, p. 2.

logical basis for separating our civilized system from that of the primitive peoples concerned.

I am profoundly impressed with the influence of the exogamous principle on primitive kinship nomenclature, but I feel strongly that the principle has not yet been formulated with adequate precision and due regard to coördinate principles of a different type. It seems to me that most writers on the subject suffer from the familiar disease of conceptual realism: the concept "classificatory system" is for them a sort of Platonic idea in the essence of which particular systems of this type somehow participate.

Even Dr. Rivers cannot be freed from this charge. In practice he does not treat classificatory systems as fully determined by the clan or gentile factor, but smuggles in additional elements that go hand in hand with exogamy in moulding relationship nomenclature. Most important among these is the principle, so strongly emphasized by Cunow, that members of the same generation are classed together and apart from other generations. Dr. Rivers regards this as so general a feature of "classificatory" systems that departures from the rule at once elicit from him special hypotheses.⁸ Again, in a concrete illustration of his theory, he has it that in tribes possessing a classificatory system a person will apply a single term to all the members of his father's clan *of the same generation as his father*. I am certainly in favor of considering clan, generation, and other causes as jointly operative in the development of kinship nomenclature, but if this method is accepted an attempt must be made to indicate the interaction of these several principles. The fact is that the mode of interaction for the two factors that are here taken into account *varies*. In some cases, Dr. Rivers' statement, that members of the same clan (or gens) *and* generation are united, holds. But in other cases, for example among the Tewa, Crow, Hidatsa, and Tlingit, the exogamous principle predominates and overrides the generation category. Here, then, is an empirical problem, to be settled for every people and only obscured by the characterization of classificatory systems generally as "clan" systems; to wit, the problem how the exogamous group is coördinated with other principles of classification.

⁸ *Ibid.*, pp. 29-31.

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The fact that Dr. Rivers has not attempted to evaluate the several factors that together determine primitive relationship terminology has led him into the curious position of underestimating in practice the very factor that occupies the dominant position in his theory. Again and again he invokes special social usages to account for "relatively small variations of the classificatory system" that are at once explained by the prepotency of the exogamous principle. For example, he cites the East Indian term *bahu*, which is applied to the son's wife, the wife, and the mother; and in explanation of this classification he assumes a one-time form of polyandry in which a man and his son had a wife in common.⁹ This assumption is *de trop* because with a dual organization and paternal descent, I and my son belong to my father's moiety, while my mother, my wife, and my son's wife must belong to the complementary moiety; hence, *bahu* may simply connote females of that exogamous group. Again, Dr. Rivers cites the Pawnee use of one term for the wife and the wife of the mother's brother, explaining this by a special form of marriage.¹⁰ But, given a dual organization with maternal descent, I and my mother's brother are members of the same moiety, while my wife and his wife are fellow-members of the complementary moiety. Finally, the confusion of generations in the Banks Islands¹¹ requires no special hypothesis. With maternal descent, my father's sister's son is classed with my father because, as among the Tewa of Hano, he is my father's clansman. My mother's brother's children are classed with my children because my mother's brother, being my clansman, is my brother; and because two brothers regard each other's children as their own. Thus, in the first confusion of generations the clan principle alone has been operative; in the second case, the clan principle has established a relationship from which a really non-existent distinction of generation is the logical derivative.

Thus, on the one hand, the exogamous theory does not suffice to explain the "classificatory" systems in their totality; on the other hand, it eliminates certain auxiliary hypotheses considered

⁹ *Ibid.*, p. 90.

¹⁰ *Ibid.*, pp. 53-54.

¹¹ *Ibid.*, pp. 28 ff.

necessary by the most eminent of its advocates. Obviously, there is something wrong with the formulation of the theory.

The solution of the difficulty lies implicitly in the original and invaluable portion of Professor Kroeber's essay on the subject¹²—the very part ignored or misunderstood by his critics—where he lists the categories in which the North American Indians have ranged relatives, and by the quantitative importance of each of which a given system may be defined. Among these categories is that of distinguishing lineal and collateral relatives,—the father from the father's brother, the mother from the mother's sister, the brother from the cousin. When we turn to Morgan's earliest description of what he afterwards took for the starting-point of his definition of the classificatory type of system,¹³ we find that what impressed him above all was the abeyance of the rule that collateral shall be distinguished from lineal relatives. This, then, forms the core of Morgan's concept, however obscured by adherent features that are logically quite unrelated. And from this point of view the Tylor-Rivers theory assumes a different aspect. Exogamy cannot explain why generations are so generally distinguished; it cannot explain the frequent differences between elder and younger *Geschwister*, or the frequent distinction between vocative and non-vocative forms; it cannot explain a hundred and one features of classificatory systems so-called. But it does explain why lineal and collateral lines of kinship are merged in the particular way characteristic of the Iroquois, Ojibwa, and many other primitive systems conforming to Morgan's Turanian type. Thus purged, the theory must now be subjected to empirical verification.

It might appear at first sight that such an empirical verification has already been given by Dr. Rivers with regard to Oceania, though this, of course, would not render it unnecessary to collect corroborative evidence from other regions. However, Dr. Rivers has in reality made a different point. In Oceania he is not dealing with classificatory and non-classificatory systems, but merely with the two forms of the classificatory system,—the Hawaiian and the

¹² A. L. Kroeber, "Classificatory Systems of Relationship," *RAI, Journal*, XXXIX (1909), 77 ff.

¹³ Morgan, *op. cit.*, Book I, chap. 4.

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Turanian. In both forms lineal and collateral relationship are merged, but in the Hawaiian nomenclature the terms are even more inclusive, no distinction being drawn between relatives of the maternal and the paternal side. Setting out with his theory and these two forms of the classificatory system before him, Dr. Rivers undertakes to show how the Hawaiian form could have developed from the Turanian form which alone follows logically from the exogamous principle. He finds that where the Hawaiian form is most clearly developed, traces of exogamy are lacking, while the highest development of exogamy is accompanied by a Turanian form of kinship system. An indefinite number of intermediate social organizations are accompanied, we are told, by intermediate kinship terminologies. The general interpretation of the phenomena offered is that a progressive change has occurred from the Turanian to the Hawaiian form, going hand in hand with the substitution of non-exogamous marriage regulations for regulation by exogamous divisions.¹⁴

This is obviously not testing the theory that the classificatory system is a function of exogamy, but merely interpreting by a special historical hypothesis the occurrence of an aberrant type of classificatory system on the supposition that the theory is already established. Granting that the hypothesis correctly represents the course of development in Oceania, we cannot assume that exogamy everywhere represents an older condition, and indeed in North America the evidence points in the opposite direction.¹⁵ Without assuming the priority of either the exogamous or the loose social organization, we can test the Tylor-Rivers theory by grouping together exogamous tribes, on the one hand, and non-exogamous tribes, on the other hand, and comparing the corresponding kinship terminologies. North America, where the geographical distribution of types of organization is fairly well determined, offers a favorable field for such an inquiry.

In the first place, there can be little doubt that the custom of identifying in nomenclature lineal and collateral relatives is very largely coextensive with the exogamous practice. It is found

¹⁴ Rivers, *Kinship and Social Organisation*, pp. 65 ff.

¹⁵ See R. H. Lowie, "Social Organization," *American Journal of Sociology*, XX (1914), 68-97, reprinted as no. 1 in this volume.

in at least three of the four main exogamous areas of the continent, —east of the Mississippi, among the southern Siouan and north-western Plains tribes, and on the Northwest coast. For the Southwest satisfactory data seem to be lacking except for the Tewa,¹⁶ where conditions are markedly anomalous. Both among the patrilineally organized Tewa of New Mexico and the matrilineal Tewa of Hano, Arizona, there are distinct terms for father's brother and father, although the term for "father" is "applied loosely to father, elder brother, father's brother, or other relatives older than self" in New Mexico,¹⁷ and at Hano to all father's clansmen, including of course his own brothers.¹⁸ For the present purpose it is important to note that at Hano it is the distinct term for "father's brother" that seems to be the older mode of designation, now rendered obsolescent by the term for "father."¹⁹ Similarly, in both branches of the Tewa, the mother's sister is carefully distinguished from the mother; in Hano, we are told emphatically, a mother's sister is never addressed as mother; and conversely we find that a woman does not address her sister's children like her own children but by a reciprocal term with diminutive suffix.²⁰ In New Mexico there is a further invasion of the exogamous principle inasmuch as no distinction is drawn between paternal and maternal uncles and aunts respectively.²¹

For the Tewa, then, the hypothetical correlation does not hold. In the New Mexican division of the people the grouping of relatives has been affected only to a very slight degree by the gentile organization. At Hano the effect of the corresponding clan organization has been greater, for among other extensions, the word for "child" is applied by a male to any of his clansmen's children and a single term embraces all the speaker's clansmen other than his own brothers. Nevertheless, even in this pueblo the divergence from the type in the designations for father, mother, and

¹⁶ J. P. Harrington, "Tewa Relationship Terms," *American Anthropologist*, XIV (1912), 472-498; Barbara Freire-Marreco, "Tewa Kinship Terms from the Pueblo of Hano, Arizona," *ibid.*, XVI (1914), 269-287.

¹⁷ Harrington, *op. cit.*, p. 479.

¹⁸ Freire-Marreco, *op. cit.*, p. 277.

¹⁹ *Ibid.*, p. 278.

²⁰ *Ibid.*, p. 276; Harrington, *op. cit.*, p. 488.

²¹ Harrington, *op. cit.*, pp. 487, 488.

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child is so great that we cannot, without doing violence to the facts, describe the kinship system as a "clan" system. In short, here is a striking instance of exogamy without an exogamous alignment of kindred.²²

A thoroughgoing explanation of the Tewa anomaly will be possible only when other southwestern systems shall have become known. For the present a few hints must suffice. So far as the Hano people are concerned, it will be well to remember that their present rule of descent, as well as parts of their kinship terminology, may be due to the influence of the surrounding Hopi and their isolation from the other Tewa pueblos. Not the inhabitants of the single village of the Tewa enclave in Hopi territory, as Miss Freire-Marreco seems to suppose, but their New Mexican congeners may use a kinship system approaching the ancient Tewan type. The problem thus narrows down to that of explaining why the patrilineal Tewa use a nomenclature that does not reflect their gentile grouping. Two alternative solutions occur to me. Either the Tewa adopted their present social organization at so recent a period that the innovation has not yet affected their mode of designating relatives. Or they have abandoned an older kinship system and borrowed a new one from some non-exogamous tribe. A third possibility, however distasteful to some minds, must be reckoned with. Though the influence of clan or gentile organization on kinship terminology seems to be a very general phenomenon, it cannot be accepted as a law of nature. It remains conceivable that a tribe should possess an exogamous social organization that finds little or no expression in the linguistic designation of kindred, just as it is now an established fact that the linguistic grouping of different relatives under the same category does not blind the users of such a terminology to the differences in the relationships.

However this may be, the single exception that has been noted cannot invalidate the empirical rule that there is a considerable correlation between exogamy and the merging of lineal and collateral relatives. Our next question is, what happens to kinship

²² This is confirmed by Morgan's statement that in Tesuque, the southernmost Tewa pueblo, all cousins alike were addressed as brothers and sisters. L. H. Morgan, *Systems of Consanguinity and Affinity* (Washington, 1871), p. 263.

nomenclature among non-exogamous tribes? The most important North American tribes without a clan or gentile organization are the Eskimo, the Mackenzie River, Plateau, and California Indians. Let us rapidly survey representative kinship terminologies of these tribes.

The Eskimo system differs so radically from the characteristic "classificatory" form that even Morgan claimed at best only a remote relationship between the two. The father's brother is distinguished from the father, the mother's sister from the mother, the children of my father's sister and of my mother's sister are designated by a common term, and so forth.²³ For the Mackenzie River district Morgan's informants seem to establish the presence of fairly well defined exogamous kinship features among these non-exogamous peoples; however, in contravention of such a system cousins are uniformly addressed as brothers and sisters.²⁴ Moreover, the exogamous features may be the result of borrowing from two distinct sources,—the Algonkian tribes to the south, and the Pacific coast population to the west. The striking coincidence of certain Northern Athapascan with Algonkian traits is noted by Morgan himself. Passing to the Salish tribes of the interior of British Columbia, we find a marked departure from the exogamous type of nomenclature. The Coast Salish draw no distinction between cousins on the father's and the mother's side; class together paternal and maternal uncles and aunts, distinguishing them from the parents; and distinguish children from all nephews and nieces. The Bella Coola likewise distinguish uncles and aunts from parents, and class together those of the paternal and those of the maternal line. Among the Shuswap there seem at first sight to be some "classificatory" features inasmuch as nephews are classed with sons, and nieces with daughters; but they are classificatory only in an etymological, not in Dr. Rivers' sense of the word, since the brother's and the sister's children are included in the same category. Boas points out that, while the Shuswap distinguish the parents from their brothers and sisters,

²³ Morgan, *Systems*, pp. 276-277.

²⁴ *Ibid.*, pp. 234-340. In Chipewyan, according to Le Goff (*Grammaire de la langue montagnaise*, p. 330), a distinction as to father's and mother's side is made when the speaker addresses a cousin of the opposite sex.

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the term used by boys for uncles coincides with the stem for "father" in other dialects, while that used by girls for aunts approximates the stem for "mother" in other branches of the Salish stock. This fact is of the utmost significance for a study of kinship terminology from a psychologico-linguistic point of view, but has no bearing on the present issue since, even in these instances, the uncles and aunts are not distinguished as to paternal or maternal side. Among the Okanagan this difference is indeed made, but the terms for parents remain distinct. In Kalispel Salish, likewise, the mother's sister is not confounded with the mother in nomenclature, nor the father's brother with the father; the terms applied by a female to her nephews and nieces are obviously related to those for "son" and "daughter," but again the brother's and sister's children are not distinguished. In short, the non-exogamous Salish tribes have a non-exogamous kinship system.²⁵ For California published data are meager, but Kroeber's statement that the systems of that area display a remarkable differentiation between the lineal and collateral lines supports the assumption of their non-exogamous character.²⁶ Finally, we may consider the Shoshonean family for which partial lists by Sapir are available for the Kaibab Paiute and Uintah Ute,²⁷ as well as unpublished data collected by the present writer among the Wind River Shoshone, White River (?) Ute, Southern Ute, and Northern Paiute, not to forget Morgan's imperfect presentation of the Uncompahgre system. With the single exception of the Wind River Shoshone, the kinship nomenclature of the entire stock is markedly non-exogamous: parents are distinguished from uncles and aunts, children from all nephews and nieces. Among the Kaibab maternal and paternal uncles or aunts are not distinguished, and even among the Wind River Shoshone all cousins are designated by a single term. On the other hand, all the Shoshonean systems are characterized by a feature shared with the Tewa,—the frequent use of reciprocal instead of correlative terms for the mem-

²⁵ Franz Boas, "Terms of Relationship of the Salish Languages," *Report of the Sixtieth Meeting of the British Association for the Advancement of Science*, 1890 (London, 1891), pp. 688-692.

²⁶ Kroeber, *loc. cit.*

²⁷ Edward Sapir, "A Note on Reciprocal Terms of Relationship in America," *American Anthropologist*, XV (1913), 134 ff.

bers of a related pair. We may well pause for a moment to consider the influence of historical accident on the course of theoretical speculation. It has been said by an eminent scientist that if physicists had first studied thermal rather than mechanical phenomena, heat would not have been described as a mode of motion but motion as a mode of heat. Had Morgan begun his researches in the Plateau area, we might have heard less of classificatory and more of reciprocal systems of relationship.

Summing up the result of our sketchy survey, we may say that the Tylor-Rivers theory derives strong corroboration from North American data. Despite some conflicting evidence exogamous kinship systems coincide so largely with an exogamous social organization and are so commonly lacking where exogamy does not obtain that a functional relation between the two must be regarded as more than probable.

We are thus emboldened to pursue our inquiry somewhat more rigorously. Having compared exogamous tribes as a whole with non-exogamous tribes as a whole, we may profitably undertake a more intensive comparison of narrower scope. Since a multiplicity of operative causes must be recognized, it becomes necessary to minimize all other differences save in point of exogamy for the purpose of studying the effect of that factor by itself. This may be done by grouping tribes according to various principles of classification. Within the Algonkian stock, for example, the Cree are reported to lack an exogamous organization. How, then, does the Cree kinship system compare with that of the linguistically and culturally most closely related Algonkian tribes possessing the gentile organization, such as the Ojibwa? From Morgan's data it appears that the kinship terminologies of these tribes agree very closely, both indicating the influence of the exogamous factor.²⁸ Does, then, the Tylor-Rivers theory break down? Not at all. It must simply be taken in connection with certain concrete facts. That kinship nomenclature may persist after the conditions in which it originated have disappeared, is a principle never urged more emphatically than by Morgan himself. The Cree terminology may therefore well be a survival from a former gentile organization, which is in this instance rather probable from its

²⁸ Morgan, *Systems*, pp. 204-208.

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prevalence in a number of closely allied Algonkian peoples. Again, within the same stock, the Arapaho and the Gros Ventre, sharing the essential traits of Plains culture and speaking mutually intelligible dialects, differ in point of organization; the Arapaho having a non-exogamous and the Gros Ventre a gentile system. If the Arapaho and Gros Ventre kinship systems differed accordingly, the history of the difference might be sought in the influence of the Blackfoot with whom the Gros Ventre have been in intimate contact and from whom they have demonstrably borrowed a number of cultural traits. The Arapaho and Gros Ventre systems, however, are identical, and, except for the designation of cousins, conform to the exogamous type.²⁹ Shall we be inclined, as in the previous instance, to assume a former condition of Arapaho exogamy?

This raises an important problem. Tylor was so strongly impressed with the correlation of "classificatory" systems and exogamy that he felt warranted in inferring exogamy from the presence of such a system. Nowadays, we shall hardly go quite so far. Granting that a correlation is established, it would still have to be proved that it is a hundred per cent correlation, that not only do exogamous peoples possess a corresponding kinship system but that no other cause could have produced such a system. The geographical distribution of certain relationship categories demonstrates that these categories, like other cultural traits, may be diffused by borrowing. In the paper already quoted Dr. Sapir points out that in the southwestern United States the use of reciprocal terms with diminutive suffixes to designate the junior relative is strangely similar among the Tewa and the Shoshonean tribes. The distribution of reciprocal systems is so definitely localized in North America, without being confined to members of a single linguistic family, as to become intelligible only on the hypothesis of borrowing. This being so, the confusion in language of lineal and collateral relatives may likewise have spread through historical connection, as has already been suggested for the Athapaskan of the Mackenzie River region. We shall therefore certainly be on the alert for evidence of former exogamy where the kinship system is exogamous, but we shall not accept the system as

²⁹ A. L. Kroeber, *The Arapaho*, AMNH Bull., XVIII (1902-1907), 9 f., 150.

proof of exogamy unless, as in the Cree instance, there are specific conditions to corroborate the conclusion. In the Arapaho case the conditions are not, in my opinion, of such a character. Here we have not a group of closely related tribes all of whom, with a single exception, possess an exogamous organization. The Arapaho and the Gros Ventre stand alone within the Algonkian family, and as already stated the Gros Ventre may have borrowed the gentile organization from the Blackfoot. Under the circumstances I do not pretend to give a solution of the problem but content myself with enumerating various possibilities. It may be, as Tylor would argue, that the Arapaho-Gros Ventre originally had an exogamous organization still preserved by the Gros Ventre, of which the Arapaho kinship system is a survival. Or, the as yet undivided parent tribe lacked exogamy, but borrowed an exogamous terminology from some neighboring people. Or, the parent tribe had neither exogamous divisions nor an exogamous nomenclature, but the Gros Ventre adopted them from the Blackfoot; and the Arapaho, in recent times, borrowed the Gros Ventre terminology. Or, the common terminology developed quite independently of exogamy,—to my mind the least acceptable hypothesis.

The two illustrations hitherto given of intensive comparison have been made on the basis, primarily, of linguistic affiliation. Important as such a classification must be when we are dealing, after all, with elements of speech, it is not always possible. This applies, for example, to California, where the degree of linguistic differentiation necessitates a different mode of grouping. Here we might ask, for instance, how the exogamous Miwok differ as regards kinship from their non-exogamous neighbors, such as the Maidu, Washo, or Yokuts. Similarly, in the Plains area, the system of the non-exogamous Kiowa would be of great interest for our present purpose. Again, the Pawnee seem to have an exogamous kinship system without exogamy. This *may* be a survival from one-time exogamy; but it may also be the result of borrowing, and a detailed comparison of the Pawnee system with that of all the tribes with which the Pawnee have come in contact may determine its source of origin.

Only through such intensive studies of detail shall we obtain

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an insight into the workings of the exogamous principle in its effect on kinship terminology and in its relations to other principles that may check or nullify its influence.

As has already been suggested, exogamy may do more than produce the fusion of lineal and collateral lines; as among the Hano Tewa, Crow, and other tribes, it may override the generation principle. This possibility is admitted by Dr. Rivers, who is, however, strongly inclined to explain the disregard of generations not by exogamy but by the practice of special forms of marriage. The special case of the father's sister's son being classed with the father is intelligible, we are told, if a man marries the wife or widow of his mother's brother, for thus he comes to occupy his maternal uncle's social status, and his uncle's children therefore regard him as their father.³⁰ This assumption has already been criticised from the point of view of logical method: on the principle that hypothetical causes shall not be multiplied unnecessarily I have argued that no special hypothesis should be advanced for minor variations of the classificatory system if the theory purporting to explain that system as a whole suffices to explain the variations. This argument does not of course refute the existence of special causes. How, then, can we be sure that it is the exogamous factor and not some such social usage as that suggested by Dr. Rivers that determines the classification under discussion? In the first place, it should be noted that the neglect of generations among the Tewa, Hidatsa, and Crow is not limited to the person of the father's sister's child, but that a single term is applied to the father's sister and all her female descendants, immediate and through females, *ad infinitum*. Shall we construct successive hypotheses as to forms of marriage by which a man would become the son of his father's sister's daughter's daughter and her successive female descendants when all the facts are summed up by the plain statement that there is one word for a father's clanswoman? That the clan is indeed the determining factor, is indicated by the effect of eliminating it. Among the Crow, as soon as we pass out of the clan by taking the daughter of the father's sister's *son* rather than of the father's sister's daughter, the generation factor at once enters: my father's sister's son

³⁰ Rivers, *Kinship and Social Organisation*, pp. 54, 28-31.

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is indeed my father since he belongs to my father's clan, but his daughter belongs to her mother's clan, hence is related to me only genealogically as my "father's" daughter, hence is my *sister*, not my mother or aunt.³¹

The conclusion is corroborated by other evidence. If the rule of descent is changed from maternal to paternal, kinship nomenclature should not be affected provided it has been shaped by forms of marriage, which are substantially uniform throughout the Plains area. If, on the other hand, kinship nomenclature has been shaped by exogamy the change from matrilineal to patrilineal descent³² must produce a change in the terminology since my father's sister's daughter no longer belongs to my father's exogamous division. Within the Siouan family, to which the matrilineal Crow and Hidatsa belong, there are also patrilineal tribes, of which the Omaha are the best known. Among the Omaha my father's sister's daughters are classed not with my father's sister but with my sister's children, while her husband is my brother-in-law. These facts seem to indicate the influence of the gentile factor, for my father's sister belongs to my gens and is therefore my gentile sister, although it must be noted that the Omaha distinguish the father's sister from the own sister. The Crow and Hidatsa with greater consistency class not only the mother's brother's children with the brother's children—which corresponds exactly to the Omaha usage, having regard to the given differences in descent—but also class the mother's brother with the elder brother. Thus, the change from the clan to the gens has eliminated the classification of the father's sister's children with the first ascending generation, and their classification with the first descending generation among the Omaha becomes intelligible through the gentile principle. But this principle may be proved to have been operative in a quite unexceptionable manner. The Omaha class the mother's brother's son and all his male descendants, immediate or through males, with the mother's brother. And again as soon as we pass out of the exogamous group, the terminology varies:

³¹ The Crow address the father's sister as "mother," but refer to her non-vocatively as "aunt."

³² This phrase is not to be taken as expressing the chronological order of events but simply the conditions of our *Gedankenexperiment* since we are passing from the consideration of a matrilineal to that of a patrilineal tribe.

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my mother's brother's son's son is my mother's brother, but my mother's brother's *daughter's* son is my brother since the bond between him and me is no longer gentile but genealogical, through his mother, who is my "mother."³³ According to Morgan these characteristics obtain for all the southern Siouan tribes.³⁴ In other words, where the matrilineal Hidatsa and Crow Indians class in one category the father's sister and her female descendants, immediate and through females, the patrilineal Omaha, Oto, Kansa, and other southern Siouan tribes unite the mother's brother and his male descendants, immediate and through males, *ad infinitum*. Better proof could hardly be demanded for the theory that the disregard of generations is the result of the exogamous principle.³⁵

Exogamy thus furnishes a sufficient explanation of the invasion of the generation principle as encountered in Melanesia and various North American tribes. The chief value of the theory that kinship classification has followed exogamous groupings lies, however, in another direction. It explains the remarkable resemblance between the terminologies of widely separated and quite distinct peoples without recourse to hypothetical historical connections. If we abandon Morgan's theory that the development of the family has been unilinear, with the main stages impressing their stamp on kinship nomenclature, how can we account for the far-reaching similarity between, say, the system of the Seneca of North America and that of the South Indian Tamil? So widespread a custom as exogamy is admirably fitted to explain the distribution of the lineal-collateral category, and it seems eminently worth while to extend the verification of the Tylor-Rivers theory both extensively and intensively. Such a study will be far from exhausting the subject of kinship nomenclature. The merging of lineal and collateral relationships constitutes but one of a number of categories, the geographical distribution of each of which must

³³ J. O. Dorsey, "Omaha Sociology," *BAE, 3d Ann. Rept.* (1884), p. 254; Morgan, *Systems*, pp. 335-336.

³⁴ Morgan, *Systems*, pp. 178-179.

³⁵ In comparing the Choctaw with the Omaha system of kinship Dr. Kohler has called attention to the influence of the rule of descent on nomenclature. See Josef Kohler, "Zur Urgeschichte der Ehe," *Zeitschrift für vergleichende Rechtswissenschaft*, XII (1897), 187-353, esp. p. 303.

be definitely ascertained. Moreover, we are sadly in need of the intensive investigation of particular systems, giving all the connotations of every term, and indicating by comparison with closely related systems how and why kinship nomenclature changes. A comparative study of all the Siouan, or all the Athapaskan, or all the Southwestern systems would be of the greatest value in this respect. However, the connection between exogamy and the "classificatory" system often hinted at but never systematically examined before Dr. Rivers' investigations in Oceania constitutes even by itself a problem of great significance and its partial solution cannot help but to react on a study of other phases of the whole question of kinship terminology.

Historical and Sociological Interpretations of Kinship Terminologies

STUDENTS OF KINSHIP TERMINOLOGY HAVE BEEN INTERESTED almost exclusively in the *sociological* inferences that may be derived from systems of relationship. They have generally failed to note that Morgan himself drew not merely sociological, but also startling *historical* conclusions from the observed phenomena. Indeed, in this regard Morgan may fairly be said to out-Graebner Graebner. He not only rejects the hypothesis that similarities in relationship nomenclature can be explained by independent development, but also summarily dismisses the suggestion of diffusion by borrowing: nothing will do but racial affiliation.

In other words the Turanian and Ganowanian families drew their common system of consanguinity and affinity from the same parent nation or stock from whom both were derived, etc. . . . When the discoverers of the New World bestowed upon its inhabitants the

Holmes Anniversary Volume: Anthropological Essays Presented to William Henry Holmes . . . December 1, 1916, by His Friends and Colaborers (Washington, 1916), pp. 293-300.

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name of *Indians*, under the impression that they had reached the Indies, they little suspected that children of the same original family, although upon a different continent, stood before them. By a singular coincidence error was truth.¹

This extravagant view was quite correctly criticized by Lubbock when he pointed out that the Two-Mountain Iroquois can hardly be recognized as more closely akin to remote Oceanian tribes than to their fellow-Iroquois. But while this criticism (with an indefinite number of similar instances that lie at hand) eliminates the use of kinship terminologies for ascertaining *racial* affinity, it does not dispose of them as evidence of *cultural* connection. Indeed, Morgan himself repeatedly cites resemblances that become intelligible only from this point of view; yet with that characteristic lack of the logical sense that detracts so largely from his otherwise superb pioneer achievements he fails to see the bearing of his data. Professor Kroeber—the only writer since Morgan who has departed fundamentally from the sociological point of view—assumes an historical position when he points out that differences in terminology are regional; but so far as I can see, he does not stress the obvious conclusion that the similarities within a given region are due to historical connection.²

In the following pages I will cite a number of striking similarities that are explicable as the result of historical connection and will discuss the relation of these facts to a sociological interpretation.

While the differentiation of elder and younger brothers and sisters is of very common occurrence, a tripartite classification of *Geschwister* is not found, so far as I know, except among the Eskimo. The Alaskan Eskimo, according to notes supplied by Dr. E. W. Hawkes, have distinct terms for elder brother, younger brother, and youngest brother. Corresponding to this, we find among the Chukchee three distinct terms for eldest brother, middle brother, and youngest brother; and a similar nomenclature among the Koryak.³ The peculiarly restricted distribution of the

¹ L. H. Morgan, *Systems of Consanguinity and Affinity* (Washington, 1871), p. 508.

² A. L. Kroeber, "Classificatory Systems of Relationship," *RAI, Journal*, XXXIX (1909), 81.

³ M. A. Czaplicka, *Aboriginal Siberia* (Oxford, 1914), pp. 30, 35.

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phenomenon is such as at once to suggest diffusion. Here, however, an alternative explanation may be given. Though diffusion be the ultimate cause, the immediate antecedent of the similarity may be similar social conditions. In other words, it is certain social peculiarities relating to the status of eldest and youngest brothers that may have been borrowed and the kinship terms may have developed independently from these borrowed usages.

A like perplexity confronts us when we consider the systems found east of the Mississippi and some of the adjoining Plains territory to the west. It is true that a characteristic trait of these systems—the merging of collateral and lineal kin—occurs in a continuous, though vast, area so that its distribution would, according to accepted criteria, be explained by dissemination from a single source. However, it is also true that this area is practically coextensive with the Eastern area of clan exogamy. The hypothesis is, therefore, *a priori* tenable that clan exogamy was the feature diffused, from which a corresponding kinship nomenclature developed independently in a number of cases. We must eliminate such possibilities if we are to establish the historical significance of kinship terms themselves.

Among the numerous tribes in the eastern and central United States which designate collateral kin by the terms used for lineal relatives there are nevertheless certain far-reaching differences connected with the tendency to recognize or to ignore differences of generation. Mr. Leslie Spier's as yet unpublished researches in this field indicate that this tendency appears primarily in the designation of cross-cousins. This phenomenon had not escaped the attention of Morgan.⁴ As he points out, the Seneca, Ojibwa, and Dakota designate a mother's brother's and a father's sister's child as "cousin"; the Southern Siouans (and Winnebago) call the mother's brother's son "uncle" and the father's sister's son "nephew"; the Crow and Hidatsa class the former with the son and the latter with the father; and at least part of the Crow-Hidatsa scheme occurs among the Choctaw and related tribes.⁵

I have elsewhere shown that the striking difference between the Southern Siouans and their northern congeners, the Crow

⁴ Morgan, *op. cit.*, p. 189.

⁵ For the sake of simplicity I cite only some of the relevant data.

and Hidatsa, is a function of the difference in rules of descent.⁶ Owing to the very close linguistic affiliation of the Crow and Hidatsa, most ethnologists will agree that their kinship systems (which coincide in many points besides those cited) were neither borrowed from each other in recent times nor arose independently, but are survivals from the system of the parent tribe, which pristine system, then, expressed the maternal (clan) organization. For like reasons, a corresponding conclusion will be deemed permissible for the Omaha and their immediate relatives, whose pristine system, as already noted by Kohler and Cunow, reflected their *paternal* (*gentile*) organization.

But as the example of the matrilineal Seneca and the patrilineal Ojibwa indicates, the rule of descent *need* not be reflected in the kinship nomenclature; for here we have tribes of different rules of descent with the same mode of designating cross-cousins, who are *not* placed in a generation above or below that of the speaker. I will not now attack the more general problem why certain tribes emphasize rules of descent while others do not. I prefer to render the problem more specific, and therefore more amenable to solution by confining attention to a single linguistic stock and to a single branch of that stock, among whose members there is a cultural bond as well. Of the Central Algonquians, the Miami, Sauk and Fox, Kickapoo, Menomini, and Shawnee agree among themselves and differ from the Ojibwa in classing the mother's brother's son and father's sister's son with the uncle and nephew, respectively; the corresponding female cousins with the mother and daughter, respectively.⁷ But this is precisely like the system of the Winnebago and Southern Siouans, as Morgan himself expressly states! Why do the Algonquian terminologies cited resemble those of a group of Siouan tribes rather than the Ojibwa nomenclature?

In order to appreciate the significance of the actual facts, we must plot them on some such map as that of Mooney in his paper "The Cheyenne Indians."⁸ We then find that the system in ques-

⁶R. H. Lowie, "Exogamy and the Classificatory Systems of Relationship," *American Anthropologist*, XVII (1915), 237 f., reprinted as No. 2 in the present volume.

⁷Morgan, *op. cit.*, pp. 211-217. My attention was drawn to these facts by Mr. Leslie Spier.

⁸James Mooney, *The Cheyenne Indians*, AAA, Memoirs, Vol. I, Part VI (1907).

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tion is spread over an absolutely continuous area, covering the territory of the Menomini, Winnebago, Iowa, Omaha, Ponca, Oto, Kansas, Osage, Illinois, Sauk and Fox, Miami, and Shawnee. But the Ojibwa also form part of a fairly continuous area,—that including besides their own habitat that of the Cree, Dakota, Wyandot, and Iroquois, all of whom designate cross-cousins as cousins. Since within both areas members of distinct linguistic families share the same kinship features, the similarities, unless explicable by similar social conditions, can be explained only by diffusion.

The similarities cannot be explained by similar social conditions. The Ojibwa do not possess either the clan or the moiety system of the Iroquois, yet they have a similar nomenclature. They do possess the gentile organization of the Central Algonquian, yet that similarity was not an adequate cause for the production or maintenance of a system similar to that of the Central Algonquians. No sociological condition can be conceived that might account for the empirical distribution of traits. On the other hand, that distribution corresponds so closely to the criterion ordinarily demanded for a proof of diffusion that diffusion, and diffusion only, must be accepted as the explanatory principle.

For the present discussion the distribution of systems with a clear development of reciprocal terms is important. To choose a common example, grandparent and grandchild are designated in these systems by a common term, or at least by a common root to which a diminutive affix is attached to distinguish the junior relative. So far as I know, systems in which reciprocal terms figure at all conspicuously are completely lacking in the Eastern Woodland, Southeastern, and Plains areas. On the other hand, they have been recorded among the Lillooet, Squawmish, Okinagan, Spokane, and Nez Percé;⁹ the Wishram, Takelma, Uintah Ute, and Kaibab Paiute;¹⁰ the San Carlos Apaches;¹¹ the Nav-

⁹ For the first three, only so far as great-grandparent and great-grandchild are concerned. Franz Boas, "Terms of Relationship of the Salish Languages," *Report of the Sixtieth Meeting of the British Association for the Advancement of Science*, 1890, pp. 688–692; Morgan, *op. cit.*, pp. 245, 249.

¹⁰ Edward Sapir, "A Note on Reciprocal Terms of Relationship," *American Anthropologist*, XV (1913), 132–138.

¹¹ Oral communication by Dr. Goddard.

aho;¹² the Zuñi;¹³ the Tewa, Acoma, and Cochiti;¹⁴ the Papago;¹⁵ the Yokuts;¹⁶ the Kern River and Mono;¹⁷ the Paviotso, Moapa and Shivwits Paiute, Wind River and Lemhi Shoshone;¹⁸ and the Kootenay.¹⁹ Having regard to our very meager information on some of the tribes concerned, everyone must again be impressed with the fact that the trait discussed is spread over a large, practically continuous area, among more than half a dozen distinct linguistic families, while outside that area it is lacking.

This phenomenon of distribution cannot be explained sociologically. The tribes cited vary fundamentally in social organization and usage. Nothing could be more distinct in this regard than the clan division of the Pueblo tribes and the loose organization of the Plateau Shoshoneans. Moreover, no social usage that could conceivably unite grandparent and grandchild has ever been reported in North America, no trace of anything like the Australian class system being known.

A more minute examination supports the theory of diffusion. Sapir has shown that "in this matter of relationship terms two such closely related dialects as Ute and Southern Paiute differ on a point on which they respectively agree with a neighboring Shoshonean and with a non-Shoshonean language (Tewa). Here, as often, a cultural dividing line runs clear across a homogeneous linguistic group."²⁰ Similarly, Mr. Gifford informs me that the Kern River people share with the Southern Paiute the use of diminutive suffixes for the junior relative designated by a reciprocal term. These tribes, though belonging to distinct branches of the Shoshonean family, are geographically contiguous. Why

¹² Franciscan Fathers, *An Ethnologic Dictionary of the Navaho Language* (St. Michaels, Ariz., 1910), pp. 435-436; personal communication by Dr. Elsie Clews Parsons.

¹³ Communication by Dr. Kroeber.

¹⁴ Communications by Drs. Parsons and Radin; J. P. Harrington, "Tewa Relationship Terms," *American Anthropologist*, XIV (1912), 472-498.

¹⁵ Dr. Kroeber.

¹⁶ A. L. Kroeber, *The Yokuts Language of South Central California*, Univ. Calif. Publ. Amer. Arch. and Ethn., II (1907), 240.

¹⁷ Mr. Gifford.

¹⁸ My own field notes.

¹⁹ Professor Boas.

²⁰ Sapir, *loc. cit.*

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they should agree in a feature not common to the entire family is not at all clear unless we assume that the similarity is a contact phenomenon. Here again a sociological interpretation is barred at the outset: what social usage or institution *could* lead to the employment of a diminutive suffix?

I can refer only briefly to certain other points. The occurrence of the distinction between vocative and non-vocative forms is one of the phenomena that should be investigated and plotted on a map. It is markedly developed among Siouan tribes, but has also been noted by Uhlenbeck for the Blackfoot,²¹ by Morgan for the Nez Percé and Yakima,²² by Sapir for the Wishram. Before adopting any interpretation we must determine more definitely the distribution of this trait.

Another very interesting feature is the differentiation of maternal and paternal grandparents. This is so completely lacking in the immense area east of the Rocky Mountains that Morgan has not even distinct tables for these relationships and notes their discrimination with some surprise for the Spokane.²³ Yet in the Far West of the United States it is exceedingly common. Boas notes it for the Kalispelm and Okinagan,²⁴ it occurs probably among the majority of Plateau Shoshoneans,²⁵ among the Takelma and Wishram,²⁶ and according to Professor Kroeber is widespread in California. According to Harrington and Kroeber, it exists also among the Tewa and Zuñi; weakly developed, it occurs among the Navaho. It is a striking fact that in the North American areas in which clans, gentes, and moieties occur (including the Tlingit and Haida) and where accordingly a discrimination between maternal and paternal grandparents might a priori be expected, such a distinction exists only in the Southwest, while the discrimination occurs precisely in the region without definite social organization. We are clearly dealing with an historical problem.

Still another peculiarity may be mentioned here because its

²¹ C. C. Uhlenbeck, "Exogamy of Peigans," *Internationales Archiv für Ethnographie*, XX (1912), 205.

²² Morgan, *op. cit.*, pp. 249 f.

²³ *Ibid.*, p. 247.

²⁴ Boas, *loc. cit.*

²⁵ Sapir, *loc. cit.*, and my own field notes.

²⁶ Sapir, *loc. cit.*

occurrence seems restricted to the same general area—the change of terms after the death of a near or connecting relative. This has been found by Professor Kroeber among the Yokuts and by Mr. Gifford among the neighboring Kern River and Kawaiisu Shoshoneans of California; Boas recorded it for several Salish tribes, the Chinook, and the Kootenay.²⁷ Again one cannot think of a social custom prevalent among these tribes capable of producing the terminological feature and lacking among tribes without it.

Finally, I may refer to the absence of separate terms for elder and younger brothers and sisters. The failure to distinguish these differences within one's own generation marks the Pawnee system,²⁸ and occurs in that of the Kiowa.²⁹ The fact that so glaring an anomaly from the point of view of the ordinary North American system should occur in the same portion of the Plains area is hardly without significance, and, once more, we are tempted to ask what social usage can be lacking here that unites the rest of the North American tribes.

The foregoing remarks seem to me to establish the principle that features of kinship terminology are distributed like other ethnographical phenomena and must be approached in the same spirit. Like specific customs, beliefs, or implements, particular features of kinship nomenclature are sign-posts of cultural relationship. This being so, it becomes obvious that in order to get the full benefit of relationship systems for historical purposes it does not suffice to record the fundamental elements of a terminology. On the contrary, as in other cases apparently trifling features are the most important because when they are found to occur in distinct tribes a *specific* historical connection is indicated. It is likewise clear that after such a general qualitative orientation as I have given above, far more detailed and in some measure quantitative studies along lines suggested by Professor Kroeber must set in. It is not the same thing whether half the terminology of a tribe is reciprocal or whether reciprocity is expressed only for the paternal grandfather and the son's son; nor whether classifica-

²⁷ Boas, *loc. cit.*; *idem*, "The Vocabulary of the Chinook Language," *American Anthropologist*, VI (1904), 135; and manuscript notes.

²⁸ Morgan, *op. cit.*, p. 197.

²⁹ My own field notes.

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tory terms (in an etymological sense) exist like our own "cousin" and "uncle" or whether the classification includes father's brothers with the father, as in many primitive systems. A much finer statistical treatment is likely to reveal the accentuation of traits in a particular center of distribution and their gradual diminution as we pass toward the periphery of the area in question. If, as appears rather likely from certain indications, the entire western slope of the United States should then be found to differ markedly from the rest of the continent in point of kinship terminology, that result would be of considerable historical significance, over and above the value of particular historical inferences.

It remains to say a word on the relation of the historical to the sociological point of view. In a paper already cited I have expressed my belief in the validity of sociological interpretation, and since I still adhere to this conviction I feel it incumbent to harmonize with it the apparently contradictory argument of the preceding pages.

As stated above, I am in favor of dealing with kinship terms in the same way as with other ethnographic features. In the interpretation of cultural resemblances the criterion for historical connection generally applied by American investigators is occurrence in a continuous area or among tribes of known historical relations, while in other instances they assume independent development. This procedure does not solve all perplexities but seems the only one that can yield any assistance at all, and it is applicable in exactly the same way to the problems at hand. When I find a reciprocal system among the Yokuts and Kern River Indians, I explain the similarity by diffusion; when I find a reciprocal system among the Australian Arunta and the Kern River Indians, I do not. In this latter case the question naturally arises *how* the similarity could arise independently. If the Californians possessed a class system like that of the Arunta, I should not hesitate to ascribe the similarity to this institution; since neither they nor any other American tribes have anything of the sort, I confess that I have no explanation to offer. But we are not always so unfortunate. The fact that the Tlingit and Crow class the father's sister's daughter with the father's sister is intelligible from the common possession of a maternal exogamous grouping by these tribes. Here as among the Choctaw, certain Pueblo and some

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Melanesian tribes the clan factor has proved stronger as a classificatory device than the generation factor. Obviously clan exogamy does not furnish a complete explanation, since it does not produce the same effect among the Iroquois, to cite but one illustration. Whether we can adduce an additional sociological determinant, remains to be seen. At all events, we may say that the trait in question is a function of matrilineal descent *plus* certain unknown factors tending to an accentuation of the rule of descent. So, wherever we can connect empirical resemblances between unrelated tribes with actual social customs from which those resemblances naturally flow, we have an adequate explanation of the phenomenon of similarity and do not require recourse to borrowing or cultural relationship.

As everywhere, so here there will be room for doubt and subjective interpretation. For example, who would state categorically that the clan system of the Muskhogean did or did not give rise to the merging of collateral and lineal kin independently of the development of this feature among the other tribes of the Eastern clan area? Here we find both similar conditions adequate to the production of the terminological trait *and* contiguity of territory. I can conceive the diffusion of a clan organization *with* a correlated terminology at a relatively early period over the entire Eastern area; but I can also conceive an independent development of the terminology from a mere diffusion of the clan concept (not to consider other logical possibilities). If the Tlingit and Haida developed a "classificatory" system independently, so could the Choctaw or the Iroquois. In such instances I think it best to admit frankly either that we can not make up our minds or that our theoretical preferences rest on more or less subjective grounds. In this regard the study of kinship nomenclature does not differ in the least from like investigations of other cultural elements. And I hope the foregoing remarks have illustrated the axiom, sometimes ignored by ethnologists, that every ethnological problem is primarily a problem of distribution.

The Kinship Systems of the Crow and Hidatsa

IN TWO PREVIOUS PUBLICATIONS¹ I HAVE EXPRESSED MY agreement with the view that kinship terminologies are correlated with social organization and have advanced North American data in defense of this proposition. I fully realize that in pointing out this correlation I have made merely a beginning. If the correlation is directly with exogamy, we are confronted with the difficulty that sometimes we do not know which of the two units—say, the Iroquois moiety and the Iroquois clan—is the primarily exogamous division. If, on the other hand, as I hinted in my shorter paper, the correlation is directly with divisions tracing descent in a definite way and only indirectly with exogamy, corresponding perplexities await us. Is the Hidatsa system, for instance, associated with the non-exogamous moieties of the exogamous

Proceedings of the Nineteenth International Congress of Americanists, Washington, December 27–31, 1915 (Washington, 1917), pp. 340–343.

¹ R. H. Lowie, "Exogamy and the Classificatory Systems of Relationship," *American Anthropologist*, XVII (1915), 223–239, and in *Proceedings of the National Academy of Sciences*, I (1915), 346–349; reprinted as No. 2 in the present volume.

clans, or have both factors jointly molded the nomenclature of this tribe? Similar questions arise among various tribes, for example among the Central Algonkians and in the Southeastern culture area.

The foregoing remarks rest on the assumption that social causes affect the kinship terminology. While, however, I accept this part of Dr. Rivers' theory, I believe his proposition that *all* features of kinship nomenclature are based on social conditions is without foundation, as I hope to demonstrate in the present paper.

In order not to prejudice the matter, let us assume that any given system of relationship is the result of an indefinite number of conditions. Then we can disentangle particular determining conditions by preserving the rest identical or as nearly identical as possible. Let us, then, take two tribes as closely allied as possible in history, culture, and language; and let us seek to correlate cultural differences of whatever kind occur with the observed differences in nomenclature.

The Crow and Hidatsa seem to furnish an ideal test-case. Their languages are very closely related, and the clan concept may be said to be identical. While the cultural differences are appreciable, the total absence of such is not to be expected on Dr. Rivers' theory, since otherwise the differences in terminology would remain without cause. Let us determine, then, to what extent observed terminological differences are traceable to a difference in social usage.

Before proceeding to a consideration of the differences it will be useful to summarize the most significant features the two systems possess in common. In the first place, both belong to the widespread centripetal, or, as it is more commonly called, "classificatory" type—the type in which collateral relatives are not distinguished from lineal relatives. Secondly, both differ from many systems of this type in lacking any term for uncle, cousin, nephew, and niece, the existing term for aunt (father's sister) being thus without a correlative. Thirdly, in connection with the peculiarity just mentioned, there is a merging of distinct generations. (*a*) The mother's brother is classed with the elder brother and addresses his sister's son and daughter as his younger brother and sister, respectively. This is an anomaly already noted by Morgan,

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who found nothing like it among other Indian tribes.² (*b*) Cross-cousins are not cousins as among the Iroquois and Dakota, but belong to distinct generations, the mother's brother's son and daughter being classed with the son and daughter; the father's sister's son with the father; and the father's sister's daughter (as well as all her female descendants through females) with the father's sister.³ The cross-cousin terminology of the Crow and Hidatsa is not confined to them. The Tewa of Hano call the father's sister's son "father."⁴ Among the Hopi the same usage prevails, and in addition the father's sister's daughter is grouped with the father's sister and the mother's brother's son with the son.⁵ The Tlingit have a single term for the father's sister and the father's sister's daughter, and a single term for the father's brother and the father's sister's son.⁶ Morgan himself cites the Muskogean and the Pawnee in this connection. As a representative of the former group the Choctaw are said to class the father's sister's son with the father, and (man speaking) the father's sister's daughter with the father's sister, while the mother's brother's son and daughter are classed with the son and daughter.⁷ The Pawnee class the mother's brother's son and daughter with the son and daughter, respectively; the father's sister's son with the father; and the father's sister's daughter with the father's sister, who is classed with the mother as in the Crow vocative form, but unlike the Crow non-vocative and the Hidatsa common form.⁸ The occurrence of a centripetal nomenclature with disregard of generations in the designation of cross-cousins and the distinctive grouping of the maternal uncle with the elder brother sufficiently characterize the Crow-Hidatsa system for the present purpose.

Let us now turn to the more striking differences in the nomenclature of the two tribes.

² L. H. Morgan, *Systems of Consanguinity and Affinity* (Washington, 1871), pp. 188-189.

³ For the Crow the last statement applies only in non-vocative usage.

⁴ W. H. R. Rivers, *Kinship and Social Organisation* (London, 1914), pp. 53-54.

⁵ My own field notes.

⁶ J. R. Swanton, "Social Condition, Beliefs, and Linguistic Relationship of the Tlingit Indians," *BAE*, 26th Ann. Rept. (1908), pp. 424-425.

⁷ Morgan, *op. cit.*, p. 191. It should be noted that the data associated with those cited above are anomalous and that corroborative testimony is highly desirable.

⁸ *Ibid.*, pp. 197-198.

Taking first the blood relatives of one's own generation, the Hidatsa have seven terms distinguishing the age of the speaker relatively to the person addressed or spoken of, while the Crow have only six. That is, while Crow males and females use a common term for the elder sister, *masa'ka'ata*, the Hidatsa have two terms, *matawi'a*, used by men, and *maru'u*, used by women. It is not easy to conceive a social reason for such a difference, but in this case we are fortunately not compelled to use purely negative evidence. The Crow term is not connected with either of the two Hidatsa roots or with any other Hidatsa stem of approximately the same meaning. It is, however, fairly clearly related to the non-vocative Crow designation for "my mother," *masa'ké*, from which it seems to be formed by fusion with the diminutive ending *-ka'ata*. On the other hand, the Hidatsa *matawi'a* is the exact phonetic equivalent (having regard to the existing linguistic relations of the two languages) of the Crow *bacbi'a*. Stripped of the initial possessives, we have here a common stem, *bi'a*, "woman," which in intervocalic position becomes *wi'a*. In Crow *bacbi'a* is a generic term for "my sister" or "my clan sister," regardless of age; in Hidatsa the equivalent expression has the specialized meaning of "my elder sister" (m. sp.). We are clearly dealing with a purely linguistic phenomenon: either the Crow have generalized the meaning of a stem that formerly had a more restricted meaning, or the Hidatsa have come to attach a narrower significance to the common stem. German *Hund* compared with English *hound*, and German *Dogge* compared with English *dog* are familiar examples of these processes.

In the first ascending generation of blood-kin there are several noteworthy differences. While the terms for father are coextensive, the Crow have distinct terms for men and women speaking, while the Hidatsa use a common word. It is not easy to suggest a sociological explanation. Again, while the stems for mother (vocative) are almost identical and nearly coextensive in meaning, the Crow word is applied to the father's sister for which there is a quite distinct expression in non-vocative usage, corresponding to an Hidatsa term that is used both in direct address and non-vocatively. What sociological reason could conceivably be adduced to account for the classification of the same relative with

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the mother in direct address and as distinct from the mother in non-vocative terminology? Comparison not only with Hidatsa but with Siouan nomenclatures generally seems to show clearly that we are here dealing with an anomalous extension of meaning, a word originally restricted to the mother, her sisters, and her cousins on the maternal side having come to be applied to the father's sister in direct address. Another curious usage may be mentioned before leaving this generation. In a number of cases the Crow distinguish non-vocative from vocative endings not by a change of stem but by altering terminal *-a* to *-e*. Now, while a father's brother or clansman is addressed *axé* like the father, the non-vocative is *ma'sa'ke*, and for all ordinary purposes the correlative vocative form ending in *-a* is lacking. Nevertheless, the term exists, but it is restricted exclusively to prayers to the Sun, in which case the significance given is "paternal uncle." Here once more we are obviously dealing with a linguistic phenomenon.

In the descending generations the most impressive difference relates to the designation of the grandchild. The Hidatsa use a common stem for the vocative and non-vocative, the former being *màtawapicá*, which receives a slightly different ending in the non-vocative. The Crow have the exact phonetic equivalent, *macba'píte*, but it is used only in the non-vocative; the grandson is uniformly addressed as "son," the granddaughter as "daughter." Of the facts themselves there can be no doubt; in addition to other evidence there is that of an important myth secured in text in which the hero, though named Old-Woman's-Grandchild, is persistently addressed as "son" by his adoptive grandmother. The grandson does not enter a new social category when he is addressed that makes him similar to the son and different from himself when merely spoken of. We are dealing, then, once more with a capricious phenomenon of language.

Turning to terms of affinity, we find one difference in terminology that in a certain sense is connected with a difference in social usage. A Crow man never speaks to his wife's brother's wife; accordingly, there is no vocative for this relative and non-vocatively she is grouped with the other persons with whom social intercourse is tabooed. Among the Hidatsa no such taboo exists

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for this affinity and consequently she may be addressed and is not classed with tabooed affinities but with the daughter-in-law and correlatively addresses her husband's sister's husband by the common term for father-in-law (w. sp.) and grandfather (m. sp., w. sp.). In this classification an influence of social organization is *conceivable*, inasmuch as the Hidatsa clans are grouped together in non-exogamous matrilineal moieties; a man would therefore be in the same moiety as both his wife's brother's wife and his son's wife, provided the moiety was once exogamous. For this, however, evidence is wanting. On the other hand, another difference cannot be thus accounted for. The father's sister's husband is classed by the Crow with the father, though he cannot possibly be in the same clan as the father; a plausible explanation is that this classification results from the designation for the father's sister, who is vocatively called "mother." We are dealing with a primarily linguistic phenomenon, then, as explained above. Among the Hidatsa the father's sister's husband (m. sp., w. sp.) is classed with the father-in-law (w. sp.). So far there is no particular difficulty beyond assuming *exogamous* moieties, since these relatives both would then belong to the same moiety. But the term that embraces these relatives is also applied indiscriminately to the maternal and the paternal grandfather (m. sp., w. sp.), who cannot possibly be grouped together on sociological grounds if we retain the basic hypothesis. It might be suggested that the term was originally restricted to the father's father, but this would be not only gratuitous assumption but contrary to all known Siouan usage, whether Omaha, Winnebago, Dakota, or Crow. The fact that the Hidatsa place the father's sister's husband in the same category with blood-relatives of the second ascending generation while the Crow class him with the first ascending generation is therefore not satisfactorily explained by the one important feature of social organization in which the tribes differ.

It seems unnecessary to pursue the comparison in greater detail. The most important observed sociological difference between the Hidatsa and Crow—the presence of moieties among the former—is either not correlated with the terminological differences at all, or it is correlated with them in a subordinate way,

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and then only if we make the auxiliary hypothesis, unsupported by any evidence, that the moieties were originally exogamous. On the other hand, the observed differences in nomenclature are of exactly the same type that we constantly have occasion to note in closely related languages. Thus, to turn to another portion of the Crow and Hidatsa vocabularies, the Crow word *íEXese* denotes a snake generically, but in Hidatsa it is applied to a particular species, while the Hidatsa word for snake means in Crow either a wasp or an otter. The same specialization and extension of significance has taken place in the kinship terminology. If we must recognize on evidence not presented in this paper a far-reaching influence of social conditions on kinship nomenclature, we must admit with equal decisiveness that many of its features are irreducible to a social basis and represent exclusively linguistic phenomena.

5

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ETHNOLOGISTS IN THE UNITED STATES ARE AGREED THAT the North American peoples of crudest culture are loosely organized, with the family as the basic unit; that tribes definitely organized into sibs (Morgan's gentes, clans of English writers) represent a higher cultural plane at which, however, the influence of the family is clearly discernible; that accordingly the sib is a later, superimposed product, not the invariable predecessor of the family. It remains to define the mechanism by which such a transformation might have been effected.

The sib, like the family, is a kinship group. It is at once more and less inclusive than the rival unit. On the one hand, it excludes one half of the blood-kindred—the father's side of the family in matronymic, the mother's side in patronymic societies. On the other hand, it admits on equal terms all kindred of the favored side regardless of degree and even individuals considered blood-relatives merely through legal fiction, whence the rule of sib exogamy. The sib normally embraces not merely the descendants through females of an ancestress, or through males of an ancestor, but several distinct lines of descent, which are only theoretically conceived as a single line. This particular form of

American Anthropologist, XXI (January–March, 1919), 28–40.

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inclusiveness, based on adoption, coalescence of ceremonial units, or what not, is too familiar a phenomenon to present any great difficulty to our comprehension. The real problem lies in the origin of what Dr. Goldenweiser calls the maternal and the paternal family pattern rather than in the expansion of these unilateral bodies of kindred to form larger groups of the same type and in theory identical with them.

It is my purpose to show that the characteristic features of the sib organization are in some measure prefigured among sibless tribes; that certain usages may bring about an alignment of kin such as occurs in sib systems; that the sib is in fact merely a group of kindred thus segregated and defined by a distinctive name.

In the interests of clearness it is well to define at the outset the relation of my present position to that assumed in previous publications.¹ Elsewhere I argued that the "Dakota" principle of classifying kin is logically and actually associated with sib systems and lacking in sibless tribes. Accordingly I concluded that the sib was the antecedent condition for the development of the Dakota type of relationship nomenclature. At present I should say that while the empirical correlation holds true the causal relations are to be reversed; generally speaking, a particular grouping of kin resulted in a sib system, though a fully established sib organization can and did in turn influence the nomenclature of kin.

In comparing the nomenclatures of sibless and of definitely organized tribes, we often find two characteristic differences. The former either fail to distinguish paternal and maternal relatives or they fail to merge collateral and lineal kin, or both. For example, the Coast Salish have a single term for paternal and maternal uncles, but distinguish children from all nephews and nieces. However, the terminologies of these peoples are by no means uniform and in many of them we can detect foreshadowings of the Dakota principle.

The most obvious of these is the classification not merely of

¹ R. H. Lowie, "Exogamy and the Classificatory Systems of Relationship," *American Anthropologist*, XVII (1915), 223-239 (reprinted as No. 2 in the present volume); *idem*, *Culture and Ethnology* (New York, 1917), chap. v.

kindred but of unrelated tribesmen as well, nay sometimes even of strangers, according to age. Dr. Karl von den Steinen was called elder brother by the Bakairí, maternal uncle by the Mehinakú.² That is to say, approximate age-mates are classed together except so far as they are differentiated by sex. This principle may be designated as Hawaiian, since it is most consistently followed by the Hawaiians and related Polynesians and Micronesians. Elsewhere, however, we do find suggestions of Hawaiian classification among loosely organized peoples. Perhaps the most common extension occurs in the second ascending generation, any venerable individual being addressed as a grandparent. To cite non-American examples, this is recorded for the Hottentot,³ and the Chukchi draw no distinction between grandfather and great-uncle, grandmother and great-aunt.⁴ The Chukchi nomenclature reveals other approximations to the Hawaiian pattern. There is no distinction between maternal and paternal uncles or aunts, and even those once removed are designated by the same terms. On the other hand, the Chukchi differ fundamentally from tribes following either the Hawaiian or the Dakota plan in rigidly separating the father from all uncles, the mother from all aunts.

In North America there are interesting analogies. The Wind River Shoshoni, I found, class all cousins with brothers and sisters, conforming to that extent wholly to the Hawaiian scheme; and Sapir notes the same feature for the Nootka. With the Hupa all women of the second ascending generation are grandmothers, all the old men grandfathers, all the children born in the same house one another's siblings.⁵ The Coast Salish go at least equally far. Here not only are great-uncles and grandfathers classed together and reciprocally call their own and their siblings' grandchildren by a common term, but all cousins are grouped with brothers and sisters, while a single term denotes father's and mother's siblings. One step further and in the first ascending generation, too, they would follow the Hawaiian principle; the

² Karl von den Steinen, *Unter den Naturvölkern Zentral-Brasiliens*, 2te Aufl. (Berlin, 1897), p. 286.

³ L. Schultze, *Aus Namaland und Kalahari* (Jena, 1907), p. 300.

⁴ Waldemar Bogoras, *The Chukchee*, AMNH, Memoirs, XI (Leiden, 1909), p. 538.

⁵ P. E. Goddard, *Life and Culture of the Hupa*, Univ. Calif. Publ. Amer. Arch. and Ethn., I (1903), 58.

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step, however, is not taken since uncles and aunts remain differentiated from parents.⁶

Such extensions of terms as have been cited hardly require special psychological explanation since they are not unfamiliar among ourselves. Among primitive tribes there exists the additional stimulus of a widespread and intense aversion to the use of personal names. But the tendency to designate individuals by a common term may have far greater than merely terminological significance. Because primitive peoples attach an extraordinary importance to names the more remote cousin who is *called* cousin or sister may become more closely related in thought and marriage may be tabooed regardless of degree of propinquity. This we are specifically told in the case of the Paviotso.⁷ Among the Nez Percé even third cousins were not allowed to marry⁸ and the union of second cousins roused ridicule in Thompson River communities.⁹ I conjecture that these are analogous cases.

However, the merging of remote and near collateral kin, or even of collateral and lineal lines of descent, does not suffice to pave the way for a sib organization; in addition to inclusiveness there must be dichotomy, that is, the extension must be unilateral not Hawaiian. Although our knowledge of the social organization of sibless tribes remains sadly inadequate, a number of cases can be presented in which there is definite bifurcation of blood-kindred. For the present a few illustrations must suffice; they are selected from four tribes typical of the great sibless area and representing distinct linguistic stocks.

Chinook¹⁰

mā'ma, -*ma*, *am*, father
-*motx*, father's brother
-*ta*, mother's brother

-*naa*, -*a*, mother
-*k!ōtcxa*, mother's sister
-*lak*, father's sister

⁶ Franz Boas, "Terms of Relationship of the Salish Language," *Report of the Sixtieth Meeting of the British Association for the Advancement of Science*, 1890, pp. 688 ff.

⁷ S. Hopkins, *Life among the Piutes* (Boston, 1883), p. 45.

⁸ H. J. Spinden, *The Nez Percé Indians*, AAA, Memoirs, II, Part III (1908), 250.

⁹ James Teit, *The Thompson Indians of British Columbia*, AMNH, Memoirs, I (1900), 325.

¹⁰ Franz Boas, "The Vocabulary of the Chinook Language," *American Anthropologist*, VI (1904), 135.

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Paviotso ¹¹

<i>na</i> , father	<i>pia</i> , mother
<i>hai'i</i> , father's brother	<i>pidu'u</i> , mother's sister
<i>atsi</i> , mother's brother	<i>pahwa</i> , father's sister

Pomo ¹²

<i>e</i> , <i>harik</i> , father	<i>te</i> , <i>nik</i> , mother
<i>keh</i> , father's brother	<i>tuts</i> , mother's elder sister
<i>tsets</i> , mother's brother	<i>sheh</i> , mother's younger sister
	<i>weh</i> , father's sister

Okanagan ¹³

<i>lEē'u</i> (m. sp.), father	<i>sk'ō'i</i> (m. sp.), mother
<i>mistm</i> (w. sp.), father	<i>tōm</i> (w. sp.), mother
<i>sm'ē'elt</i> , father's brother	<i>swāwa'sā</i> , mother's sister
<i>sis'7</i> , mother's brother	<i>sk'ō'koi</i> , father's sister

Such dichotomy of kin as is here indicated is exactly what might be expected under that family organization which American students regard as prior to a sib system, for since the parents belong to different families their relatives are logically enough distinguished from one another.

Let us now assume that the bifurcating and the merging tendency as hitherto expounded unite. Then we shall have a terminology in which all the mother's female kindred belonging to her generation will be classed with the mother's sister, all of her male kindred in that generation are treated as mother's brothers, while corresponding classification is given to the father's relatives. In that generation we shall have an alignment anticipating that of the Dakota type, from which it differs solely in the distinction maintained between parent and parent's sibling of the same sex.

What happens, however, in the speaker's generation? Corresponding to the four uncle-aunt terms we might logically expect an equal number of cousin terms, or even twice as many through sex discrimination. As a matter of fact, the classification of cousins follows quite different principles. In some nomenclatures

¹¹ A. L. Kroeber, *California Kinship Systems*, Univ. Calif. Publ. Amer. Arch. and Ethn., XII (1917), 359.

¹² *Ibid.*, pp. 370 f.

¹³ Boas, in "Terms of Relationship of the Salish Language," p. 691.

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of sibless tribes, e.g., the Paviotso and Shoshoni, the Hawaiian principle is applied and all cousins are brothers and sisters. Among the Coast Salish we find the same grouping but also a specific term for cousin. I assume—and this is the most hypothetical feature of my scheme—that at the stage preceding the evolution of the sib the natives had specific terms for brother and sister, while all other relatives of that generation were lumped together under a single term except so far as they were differentiated according to sex. This would yield a grouping somewhat similar to that in the first ascending generation since the members of the immediate family would be segregated from more remote kin. On the other hand, this classification would differ from that characteristic of most tribes with a sib organization. For one of the essential features of their nomenclatures lies in the dichotomy of cousins according to the likeness or unlikeness of the sex of the parents through whom the relationship is established. In perhaps the most common variety of the Dakota scheme parallel cousins are brothers and sisters, cross-cousins are designated by a distinct cousin term.

It is essential to point out that no perfectly satisfactory explanation of this classification has been given except on Tylor's hypothesis that it originated in a moiety organization.¹⁴ The hypothesis that parallel cousins are simply moiety mates admirably accounts for the grouping but does not cover the facts of distribution, since the division into parallel and cross-cousins is often found with a multiple sib system.¹⁵ This, however, in turn fails to account for the classification. If there are only two sibs in a tribe (or, prior to sibs, only two intermarrying families), cross-cousins are in one moiety and parallel cousins in the other, as Tylor pointed out. But if there are five, the condition is very different. Assuming maternal descent, the children of sisters will indeed belong to the same social unit but the children of brothers need not; one may marry into group *b*, the other into group *c* and their children will belong to their respective mothers' sibs.

¹⁴ E. B. Tylor, "On a Method of Investigating the Development of Institutions," *RAI, Journal*, XVIII (1889), 264.

¹⁵ Cross-cousin marriage, which seems closely connected with a dual organization, also has a distribution far too limited to account for the data.

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Now I assume that upon tribes bifurcating but merging relatives unilaterally in the manner described above, there are superimposed two extremely widespread customs, the levirate and the sororate. The terminological effects of these usages have been amply discussed by Sapir,¹⁶ though not quite adequately as regards cousin nomenclature. One obvious result is to obliterate the distinction between father and father's brother, mother and mother's sister. In short, the Chinook and other terminologies cited (pp. 85-86) come to conform to the Dakota principle in the first ascending generation. Since father's brother and mother's sister become parents, their children become siblings, which accounts for the grouping together of parallel cousins. But it is not clear why father's sister's and mother's brother's child so often remain undistinguished. If, however, all cousins have previously received a common designation on the basis of generation, being differentiated only from those contemporaries who form part of the narrow family circle, as I assume, then the effect of the levirate and sororate is to raise parallel cousins to the status of siblings, while cross-cousins remain in the general class of contemporaries.

I offer this suggestion not as a substitute for Tylor's interpretation but as supplementary to it; it is designed to cover those cases in which parallel cousins cannot be classed together as members of one moiety and cross-cousins of the other for the simple reason that no dual organization exists, either in a fully developed or nascent form.

The relation of these marriage customs to social organization merits some additional consideration. As to their significance I indorse whole-heartedly Tylor's interpretation that the levirate reflects a matrimonial compact not between individuals but between families; and that for lack of actual brothers more remote male relatives are substituted.¹⁷ Corresponding views of course apply to the sororate. Wherever our data are sufficiently explicit, they seem to corroborate Tylor's theory. For example, the Shasta purchase wives and a man is aided in the transaction by his broth-

¹⁶ Edward Sapir, "Terms of Relationship and the Levirate," *American Anthropologist*, XVIII (1916), 327-337.

¹⁷ Tylor, *op. cit.*, p. 253.

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ers and relatives; accordingly it is natural that they should lay claim to the widow. On the other hand, a widower or the husband of a barren woman might take as his second spouse one of his wife's unmarried sisters or cousins.¹⁸ Thompson River Indian practice closely conforms to that of the Shasta; more particularly a man held an incontestable claim to his brother's widow.¹⁹

In a discussion of Dr. Sapir's paper on the levirate²⁰ I raised certain difficulties, some of which would militate no less against the position I now assign to these usages than against Dr. Sapir's explanation of kinship nomenclatures. Probably the most important of these is a chronological one: if the levirate and the sororate developed subsequently to the sibs they could not of course give rise to that classification of kin which I now regard as underlying the sib. Now it is true that since Tylor no one has taken the trouble to ascertain the precise distribution of either custom and his concrete data are apparently lost. But in the light of my reading I am tempted to regard his result—a forty per cent distribution of the levirate among primitive tribes—as far below the figure that would be established by a count today. This seems certain for North America; and here we find the interesting result that levirate and sororate are found jointly almost throughout the great sibless area—among the Salish of British Columbia, in our Pacific states, and the Great Basin. They are thus characteristic of the simpler sibless cultures, but they also appear commonly on a higher level with the sib system. The inference is warranted that they are traits preceding the sib organization and in a manner preparing the way for it.

This, to be sure, would not apply to the Pueblo area, where neither levirate nor sororate is in vogue. But the best-known tribes of this region differ rather markedly in their nomenclature from the Dakota norm, though in a manner not inconsistent with the principles I have outlined above. The Zuñi group cousins of both sides as siblings, though applying peculiar notions in point of seniority which may here be disregarded.²¹ This is quite intel-

¹⁸ Roland B. Dixon, *The Shasta*, AMNH Bull., XVII (1907), 463 f.

¹⁹ Teit, *op. cit.*, p. 325.

²⁰ Lowie, *Culture and Ethnology*, pp. 144–150.

²¹ A. L. Kroeber, *Zuñi Kin and Clan*, AMNH, Anthropological Papers, XVIII, Part II (1917), 58.

ligible, of course, on the principle of generations. With the Hopi the two kinds of cross-cousins are differentiated (see below), so that the problem as to their classification does not arise in the usual form. But what of the Zuñi and Hopi classification of uncles and aunts? Here, too, I can see no difficulty. Though the levirate, e.g., supplies an excellent specific reason for identifying father's brother and father while differentiating them from the mother's brother, the joint force of the more general bifurcation and generation factors is adequate to produce the same result. Since father's brothers thus came to be reckoned as fathers, and mother's sisters as mothers, the Hopi classification of parallel cousins as siblings follows: the children of those I call my parents must be my brothers and sisters.

The classification of parallel cousins, however, involves a fundamental obstacle to any theory that would derive the sibs from an earlier system of kinship nomenclature. As Morgan himself pointed out, the status of sibling is not coterminous with that of sib fellow. In a matrilineal society only the children of sisters, not of brothers, belong to the same social unit, yet *all* parallel cousins are addressed as brothers and sisters.²² If we assume that the conditions described above gave rise to the terminology that normally accompanies a sib organization, then why were some of the brothers and sisters taken into the sib and others discarded?

In attempting to answer this question I desire at the outset to emphasize my belief in a multiple origin of the sib idea; even in North America I hold that there have been several centers of distribution. For one thing, I am strongly impressed with the enormous variability of the sib concept. Secondly, the generalized sib idea—unilateral descent—is not, as Morgan would have it, an abstruse quasi-metaphysical notion, but one that quite naturally develops from certain cultural features. These features, moreover, may favor either patrilineal or matrilineal descent; hence I see no reason why either father-sibs (*gentes*) or mother-sibs (*clans*) should not have arisen directly from a loose organization instead of either having to evolve out of the other, though of course I do not reject the possibility of such a transformation.

²² L. H. Morgan, *Systems of Consanguinity and Affinity* (Washington, 1871), pp. 475 f.

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To turn to the problem of parallel cousins. Sibless communities have often clear-cut regulations tending to establish definite lines of descent. The Shasta and the Thompson River Indians recognized individual ownership of fishing stations with patrilineal descent of the title to them.²³ Such possessions might not loom large enough in the tribal consciousness to lead to significant consequences, they might even be outweighed by other considerations stressing the maternal lines of descent. It is quite different when economic privileges of some consequence are involved or when there is a definite rule determining the residence of a couple after marriage, or where both these factors coöperate. For example, with the Bushmen, land descended in the paternal line; Dr. Bleek's informant occupied the site held by his father's father, which had descended first to his father, then to his elder brother, and finally to himself.²⁴ By such an arrangement sisters are separated, brothers and their descendants are united, at least through their property rights. In the permanent villages of the Hupa men were born, lived, and died in the same village, while women followed their husbands.²⁵ The paternal line of village mates was thus inevitably stressed while the offspring of sisters were scattered over different localities.

In recent years no one has emphasized the significance of such conditions for social organization more vigorously than Professor Speck. In the northeastern Algonkian region he finds non-exogamous groups transmitting hunting territories quite definitely from father to son and following patrilocal residence rules; brothers to some extent share economic privileges.²⁶ Given such customs, it will not matter whether through the levirate and sororate all parallel cousins are addressed as brothers and sisters. Those parallel cousins who live together and share the same hunting prerogatives, i.e., the children of brothers, will be automatically set apart from the children of sisters and come to be considered as in some

²³ Teit, *op. cit.*, pp. 293 f.; Dixon, *op. cit.*, p. 452.

²⁴ W. H. I. Bleek and L. C. Lloyd, *Specimens of Bushman Folklore* (London, 1911), pp. 305-307.

²⁵ Goddard, *op. cit.*, p. 58

²⁶ Frank G. Speck, "Kinship Terms and the Family Band among the Northeastern Algonkians," *American Anthropologist*, XX (1918), 143 ff.; *idem*, *Family Hunting Territories*, Canadian Geological Survey, Memoir 70 (Ottawa, 1916).

respects more closely related. I regard Dr. Speck's data as most important in demonstrating what is to all intents and purposes a nascent father-sib. The external details of the processes involved may of course vary. For example, in the region of the northwest Amazons, the social unit is the exogamous house community of as many as two hundred individuals. Residence is patrilocal so that brothers take their wives to the same house. This sets up the same difference as among the Algonkian between the two kinds of parallel cousins, and here we have the interesting phenomenon that marriage with parallel cousins from other households, i.e., unions between the children of sisters, are permitted.²⁷

In considering matrilineal societies Tylor was inclined to derive their essential features from the basic fact of matrilocal residence.²⁸ This is a luminous suggestion, for from matrilocal residence the segregation of matrilineal kin logically follows, as does the exceptional status of the maternal uncle. Nevertheless a serious obstacle to this interpretation as a general theory of the origin of mother-sibs lies in the restricted distribution of matrilocal residence even where descent is matrilineal. The Australians are practically all patrilocal, the Melanesians predominantly so, and some matronymic tribes in both Africa and America likewise have the wife living with her husband. There is the additional difficulty that residence very often is only temporarily with the wife's parents, in which case it suggests not infrequently merely an obligation on the husband's part to serve for his wife in lieu or part payment of the bride-price. Evidently if a young couple only stay with the wife's parents for a year or two and then set up an independent household, the conditions for a matrilineal reckoning of kindred are not the same as among the Hopi or Zuñi, where women own the houses and their husbands permanently reside with them. This fundamental difference between permanently and temporarily matrilocal residence still further restricts the applicability of Tylor's theory. Nevertheless it may be accepted as admirably fitting the case of the Pueblo Indians, for, as Professor

²⁷ Thomas Whiffen, *The North-West Amazons* (New York, 1915), pp. 63, 66 ff.

²⁸ Tylor, *op. cit.*, p. 258; *idem*, "The Matriarchal Family System," *Nineteenth Century*, XL (1896), 81-96.

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Kroeber has shown, the sum and substance of the Pueblo "matriarchate" lies in the female ownership of the houses.²⁹

In attempting to supplement Tylor's explanation it seems to me that attention should be specially directed to economic conditions and the sexual differentiation of labor. Eduard Hahn has familiarized us with the distinct character of horticulture and aratory culture—the former being in the hands of the women, the latter belonging uniformly to the masculine domain. Does not this suggest an interpretation of the kind required? Unfortunately we often lack details as to the manner of tillage, but recent data on the Hidatsa seem extremely suggestive. Here gardens were tilled jointly by the women of the maternal family and descended in the maternal family.³⁰ That is to say, the female descendants of sisters were actually united by common property rights and association in economic activities. The fact that male descendants are not included in these labors does not seem to me fatal, for as soon as the joint tillers were differentiated by a name their infants would automatically share the same designation from birth. It is interesting to note that in this region there is no record of individual hunting prerogatives of the males to counterbalance these horticultural privileges of the women.

I realize that my hypothesis, even when joined to Tylor's, does not account for all the cases of matrilineal sibs in the world. The patrilocal and non-horticultural Australians and Northwest Coast Indians remain to be explained. Nevertheless matrilineal residence and the joint economic activities of women suffice to account for a majority of the known cases, and the residual phenomena might at least be approached from a similar point of view.

I assume, then, that bifurcation and age-stratification, which occur among many sibless tribes, are conditions antecedent to the sib organization but produce an alignment of kin approximating that of the Dakota-Iroquois nomenclatures. The levirate and sororate, while not indispensable, render it more probable that the first ascending generation should be designated after the

²⁹ Kroeber, *Zuñi Kin and Clan*, pp. 47 f., 89 f.

³⁰ Gilbert L. Wilson, *Agriculture of the Hidatsa Indians* (Minneapolis, 1917), pp. 9 f., 113 f.

normal sib fashion; and they may further bring about the usual grouping of cousins. But in order that sibs shall develop from such a terminology, it is inevitable that the children of brothers be differentiated from those of sisters. I follow Tylor in explaining part of the phenomena by patrilocal or matrilocal residence. Others seem intelligible from the sociological differentiation of the sexes and the consequent establishment of unilateral lines of descent.

When the sib has taken firm root, it is quite possible for it to react upon the kinship terminology. Not only may the kinship idea be extended to similarly named sibs of alien peoples, but the sib affiliation may even override the basic generation scheme, as among the Crow and Omaha. In these instances, too, it is desirable to view the facts in connection with associated cultural features. Even in such cases the terminology may sometimes result from concrete social arrangements involved in the sib organization rather than from the abstract concept of the sib. For example, the Hopi classification of the father's sister with all her female descendants through females simply groups under one head a series of house mates, which manifestly does not apply to the Crow or Hidatsa.

The present is not an historical paper but a sketch intended to stimulate historical studies. If the sib is later than the family, we cannot indefinitely postpone an inquiry into the conditions that have moulded the sib out of a prior family organization. This involves the demand that we must learn a great deal more about the social life of the loosely organized peoples. The social customs of these tribes are no more uniform than are the sib organizations of other tribes. Both must be studied intensively and with constant consideration of the concomitant cultural traits if we are ever to frame a satisfactory theory of the development of social organization.

6

A Note on Relationship Terminologies

IN HIS *Systems of Consanguinity and Affinity*, LEWIS H. Morgan divides relationship systems into two types:

One of these is descriptive and the other classificatory. The first . . . , rejecting the classification of kindred . . . describes collateral consanguinei, for the most part, by an augmentation or combination of the primary terms of relationship. . . . But the second . . . , rejecting descriptive phrases in every instance, and reducing consanguinei to great classes by a series of apparently arbitrary generalizations, applies the same terms to all the members of the same class.¹

As representative of the former type, Morgan cites the Aryan, Semitic, and Uralian systems, while the American Indian (and primitive nomenclatures as a whole) illustrate the second type.

Kroeber and Rivers have criticised the basis for the distinction inasmuch as our English and other Indo-European terminologies have such classificatory terms as "uncle" and "cousin." Morgan mentions these as constituting "a limited number of generaliza-

American Anthropologist, XXX (April-June, 1928), 263-267.

¹ L. H. Morgan, *Systems of Consanguinity and Affinity* (Washington, 1871), p. 12.

tions" but feels that their use "does not invade the principles of the descriptive system and that their origin lies in the constantly recurring desire to avoid the inconvenience of descriptive phrases."² This explanation evidently does not satisfy the demands of logical classification.

Rivers also objects to the term "descriptive" as applied to the ordinary Indo-European nomenclature. When the Norwegian combines the stem for "father" or "mother" with that for "brother" to form the words *farbror* and *morbror* for the paternal and maternal uncle, he is evidently "describing" the relationship in Morgan's sense of the word. But where English does not employ classificatory terms, as in the case of "uncle," it evidently falls back for the most part upon such primary stems as those for father or mother, which cannot by any criterion be called "descriptive," but are simply, as Rivers contends, "denotative." Morgan's term, then, should be restricted to systems that actually exhibit a strong tendency to define relations by descriptive compounds.

So far as I am aware, no one has called attention to a basic logical error in Morgan's dichotomy, quite regardless of the relevant facts. "Classificatory" and "descriptive" are not complementary concepts, but belong to different logical universes: the former envisages the singularity or plurality of the kinsfolk designated; the latter considers the technique by which kinsfolk are defined. It is conceivable that a tribe should designate the paternal uncle by a descriptive phrase and apply that term to the whole class of father's clansmen within, say, the latter's generation. What is more, this might even hold for so common a concept as that of a sibling. The Ewe call a brother "mother's-child-male." What is to prevent them from applying this compound as widely as the more usual primary stem for brother? The logical complement of "classificatory" is evidently not "descriptive" but "individualizing"; the logical complement of "descriptive" is Rivers' "denotative" if the word is understood to refer to designation by primary stems. That this is not a matter of mere logic-chopping, appears from the Lango relationships, which, "though based on the classificatory system, include a number of descriptive terms

² *Ibid.*, pp. 12, 48.

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some of which, nevertheless, are used in a classificatory way.”³

In this connection it seems worth while to point out another confusion of thought. It is often stated that classificatory systems are characterized by the discrimination of elder and younger sibling. Obviously, it is the more generic terms “brother” and “sister” that come closer to the “classificatory” standards. It so happens that the discrimination frequently occurs in non-classificatory terminologies. There is thus neither logical nor empirical warrant for the correlation asserted. After the distinction has once been established, the terms can of course be extended in a classificatory sense. But the distinction as such is in conflict rather than in harmony with classificatory ideals.

Among the most lamentable phenomena in the recent literature of the subject is the tendency of British writers to speak of “Clan” and “Family” nomenclatures. I myself believe in a fairly high correlation of clan systems with a classificatory terminology of the Iroquois-Dakota type. However, correlation does not imply a hundred per cent correlation nor a cause-and-effect nexus; it means, on the face of it, a functional relationship in the mathematician’s sense. The terms here criticized are inexcusable because they prejudge a theoretical problem by injecting the *inferred* cause into the description of observed phenomena. The result is inevitably baneful. As a matter of fact, there are clanless tribes with a “Clan” terminology; and to describe them as having “Clan” systems would not be conducive to clarity.

As Kroeber long ago indicated,⁴ kinship terminologies are not so many coherent “systems” but are each founded on a variety of disparate principles, all of which must be enumerated for a complete definition. Where the mother’s sister is called “mother” and the sister’s son (woman speaking) is a “son,” these two features are parts of one system. But if they are linked with the use of separate words for “mother” by men and women, *that* is no longer part of the same organic whole. It is even virtually demonstrable that particular terminologies have become less sys-

³ J. H. Driberg, *The Lango, a Nilotic Tribe of Uganda* (London, 1923), p. 180.

⁴ A. L. Kroeber, “Classificatory Systems of Relationship,” *RAI, Journal*, XXXIX (1909), 77-84.

tematic. Thus, a term for paternal aunt implies as its logical correlate a separate term for brother's son (w. sp.), but in some tribes that term does not exist, having been lost, as we may infer from a comparison of cognate languages. That is to say, there has been a secondary departure from the systematic character of part of the nomenclature. The whole becomes proportionately harder to define in brief compass.

If the terminologies of the world were both extensively and intensively better known, it would be necessary to attempt a wholesale classification on the basis of as many categories as possible. At present this is hardly feasible, and a provisional survey of the ground is best essayed with as simple a scheme as can be applied, to wit, by taking a single significant criterion. The historical development of the subject, from Morgan down, suggests the treatment of collateral relatives of the first ascending generation as the most suitable basis. The logical possibilities are the following:

1. Uncles and aunts may be treated as parents.
2. The paternal uncle may be classed with the father, while the maternal uncle is designated by a specific term; and, correspondingly, the maternal aunt may be classed with the mother, while the paternal aunt has a specific designation.
3. The paternal and maternal uncles (or aunts) are alike distinguished from the parent *and from each other*.
4. The paternal and maternal uncles (or aunts) are alike distinguished from the parent, but bear a joint uncle (or aunt) designation.

The merging of uncles and aunts with parents constitutes a Generation terminology. If the males (or females) of the first ascending Generation are dichotomized on the principles explained, the terminology may be called Bifurcate Merging: bifurcate, because paternal and maternal kin are distinguished, merging insofar as there is a *partial* merging with the parents. Where this merging fails to obtain, so that each collateral relative is distinguished, the nomenclature becomes Bifurcate Collateral. If, finally, the collaterals are confounded with each other but remain separate from the direct line of descent, such emphasis on the latter merits the term Lineal.

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Evidently, the Generation type corresponds to Morgan's mis-named "Malayan" or Rivers' Hawaiian system; the Bifurcate Merging, to the more common "classificatory" form variously called "Turanian-Ganowanian," "Clan," "Dakota-Iroquois"; the Lineal, to the common Indo-European (Morgan's Descriptive, Rivers' Family) system. The Bifurcate Collateral, a second "Family" system, has been generally ignored by theorists, though its presence in North America has been repeatedly noted.

The designations here employed are awkward but serve to bring out connections usually disregarded. Specifically, Morgan and Rivers stressed the genetic relationship of Generation and Bifurcate Merging terminologies: Morgan traced the development of the latter from the former; while Rivers reversed the process. The recognition of Bifurcate Collateral terminologies opens a new prospect,—the derivation of the Bifurcate Merging from the Bifurcate Collateral type. Logically, the affiliation is not one iota smaller than between the two systems compared by Morgan and Rivers. Empirically, and bringing in sociological correlations, marriage is even in clanless societies often a contract between two families, whose separateness is emphasized and may thus find expression in language. When, for some reason—say, the joint prevalence of the levirate and the sororate—partial merging develops, the Bifurcate Merging type would come into being.

The Omaha and Crow Kinship Terminologies

IN HIS SURVEY OF NORTH AMERICAN KINSHIP NOMENCLATURES Lewis H. Morgan discovered that a series of Southern Siouan tribes, including among others the Omaha, Iowa, and Osage, as well as the Siouan Winnebago and a series of Algonkians, including among others the Illinois, Sauk-Fox, and Shawnee, did not classify cross-cousins according to the Iroquois terminology most familiar to him.¹ Instead of designating cross-cousins by a distinctive word, these tribes put them either into the first ascending or into the first descending generation. The mother's brother's son was called by the same term as the mother's brother; indeed, this term was applied to all male descendants of the maternal uncle through males, irrespective of generation. Correlatively, the father's sister's son was equated with the sister's son, which meant that a man called this cross-cousin by a distinct nephew term, while a woman called him her son. The mother's

Verhandlungen des XXIV. Internationalen Amerikanisten-Kongresses, Hamburg, 7. bis 13. September 1930 (Hamburg, 1934), pp. 103-108.

¹ L. H. Morgan, *Systems of Consanguinity and Affinity* (Washington, 1871), pp. 179, 217.

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brother's daughter was classed with the mother and logically called her father's sister's children as she would her own children.

These findings were not only corroborated by later investigators so far as the same tribes were revisited, but a second centre of distribution was discovered by E. W. Gifford in California.² It takes in the Miwok, Wintun, and part of the Pomo and Yokuts. Gifford's and Spier's maps³ show that within these two widely separated areas the distribution is absolutely continuous. The two groups in question stand out sharply from all neighboring tribes by their peculiarities in kinship classification and are thus rightly segregated by Spier as constituting a distinct "Omaha" type. There is only one conceivable explanation of the distributional data—historical connection within each of the two areas. Further, we must note that the grouping does not coincide with linguistic classification: only *some* of the Siouans and *some* of the Algonkians help constitute the Omaha type in the East, while in California the Southern Pomo and the Wappo are separated from the other Pomo divisions. In other words, while the distinctive features of the type may go back to inheritance of an ancestral system so far as the Southern Siouans *or* the Central Algonkians in question are concerned, there must have been borrowing by the former from the latter, or vice versa; and similarly for the California tribes.

On the other hand, nothing warrants us in assuming that the Californians borrowed their system from the East or the Easterners theirs from California. There is no indication that the Miwok and the Omaha were ever nearer to each other than they are now, and none of the intervening tribes show a trace of the features peculiar to their nomenclature. This conclusion, however, fails to shed any light on the resemblance. How could such an anomalous set of features, one in apparent contravention of common sense, come into being in two distinct regions? Indeed, the query may be widened geographically, for from outside America at least two clear-cut parallels are available. The Lhota

² E. W. Gifford, *Miwok Moieties*, Univ. Calif. Publ. Amer. Arch. and Ethn., XII (1916), 172; *idem*, *California Kinship Terminologies*, *ibid.*, XVIII (1922), 163.

³ Leslie Spier, *The Distribution of Kinship Systems in North America*, University of Washington Publications in Anthropology, I, No. 2 (1925), 69–88.

Kinship and Social Organization

Naga of Assam apply the word "omo" to the mother's brother and to the mother's brother's son; and as among the Miwok the mother's brother's daughter is classed with the mother's younger sister. Further, the father's sister's son and the sister's son are both designated as "orrho."⁴ The Lango of the Upper Nile likewise have one word, "okeo," for the sister's and the father's sister's son of a man, though they do not merge the concepts of maternal uncle and maternal uncle's son.⁵ One Asiatic and one African parallel are thus added to our list, and we are forced to speculate as to the conditions that might favor the recurrence of such oddities of terminology in four remote centres.

What, then, is the element of social organization that allies the two groups of American tribes, the Assamese, and the Nilotics? A glance at Professor Spier's roster at once shows that virtually every one of the American tribes is patrilineal, and this likewise holds for the Naga and the Lango. The only exception is provided by the four subdivisions of the Pomo, but even for them no one has claimed a full-fledged maternal clan system. The most that can be asserted is that the Pomo have matrilineal *tendencies*. Besides, the proximity of tribes having the Miwok peculiarity would amply account for its extension to the Pomo by diffusion.

A patrilineal system would inevitably place in the same clan or lineage the maternal uncle and his son; the mother, mother's sister, and maternal uncle's daughter; the sister and the father's sister; and as a corollary from the last equation the daughters of these relatives might also easily be treated as identical. The aberrant classification is thus apparently nothing but an overriding of the generation principle in favor of the clan or lineage principle: kinsfolk, even though not of the same generation, are designated by one term if in the same unilateral group or if offspring of members of the same group.

It is possible here to apply the logic of the "control" method characteristic of the exact sciences. Let us eliminate the clan factor and see what happens. The Omaha, as shown above, recognize an infinite series of maternal uncles, so that the mother's brother's son's son is likewise a maternal uncle. Let us, then, com-

⁴ J. P. Mills, *The Lhota Nagas* (London, 1922), p. 93.

⁵ J. H. Driberg, *The Lango* (London, 1923), p. 177.

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pare with this the designation for the mother's brother's *daughter's* son. We are keeping the sex and generation of the relative the same, only varying his clan affiliation, since paternal descent would place him in a different clan from his mother's. The Omaha respond to the change of stimulus with the precision of an automaton. The mother's brother's daughter's son is *not* a mother's brother. His status is determined by the simple fact that his mother is considered the speaker's "mother," hence he becomes a brother.

Mother's brother's daughter = mother

(Mother's brother's daughter)'s son = mother's son

Whence, by substitution: mother's son = brother

But while the Omaha-Miwok type thus emerges as functionally connected with paternal descent, paternal descent by itself cannot be regarded as an adequate determinant. In America there are patrilineal tribes, like the Cree and Ojibwa, who do not share the Omaha peculiarities and, as a matter of fact, are aligned with matrilineal tribes like the Seneca and the Tsimshian.⁶ We must accordingly seek an additional factor that might effect the observed phenomenon.

The supplementary determinant required appears both among the Omaha and the Miwok. The latter allowed a man to marry his wife's brother's daughter before or after his first wife's death.

"In some cases, if she were too young for him to marry, she was held for him until she had reached the marriageable age, when she was handed over to him."⁷

Similarly, an Omaha may marry his wife's brother's daughter (as well as her father's sister or her sister, elder or younger).⁸

Gifford has pointed out how an institution of this sort would affect terminology⁹ and a variant of his diagram will serve to illustrate the point.

Here, the small letters indicating females and capitals males, A appears as the husband of both b and her niece e. Let us then view the situation from the angle of the children he begets with the younger wife. For g or H, F = a mother's brother (literally).

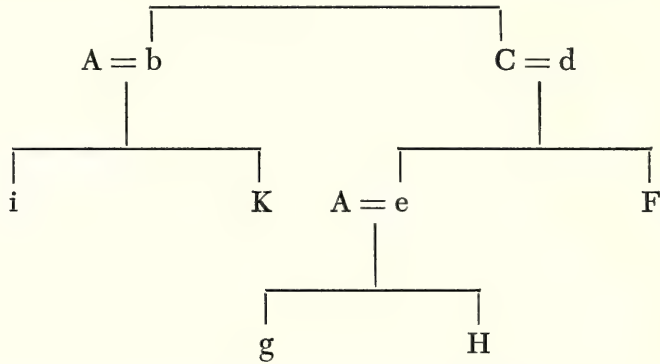
⁶ Spier, *op. cit.*, p. 78.

⁷ Gifford, *Miwok Moieties*, p. 186.

⁸ J. O. Dorsey, "Omaha Sociology," BAE, 3d Ann. Rept. (1884), pp. 257, 261.

⁹ Gifford, *California Kinship Terminologies*, p. 248.

But since A is married to b likewise, b as well as e figures as their mother, hence C no less than F is their "mother's brother." But F is literally C's son. Hence, F is at the same time g's and H's maternal uncle *and* their maternal uncle's son; which was to be proved. Further, since e has become A's wife, the children i and K, whom he has begotten with b, become e's children. Shifting, then, the point of origin to F, his sister's children in this extended sense are identical with the actual children of his father's sister.



Mr. Driberg has shown that the Omaha-Miwok feature shared by the Lango has a different origin, so that we apparently meet here one of those rare instances of demonstrable convergence. Like many African Negroes, the Lango practise filial succession as to widows. Accordingly, a man F will anticipate his status as d's husband, equate himself with his father, hence look upon b as his sister, and upon b's children as his sister's children. Of course, the widows inherited do not include the actual mother, but a father's wife addressed as mother.¹⁰

The forms of marriage deemed orthodox by the Lhota are very interesting. A man may take his brother's widow without bride price, is expected to marry into his mother's clan and is fined "if, having taken one wife from his mother's clan, he takes a new one from another clan." He may marry his mother's brother's daughter but not the other cross-cousin; we do not learn whether his wife's brother's daughter is a possible mate. However, "He may also marry his father's widow provided that she is not his own mother,

¹⁰ Driberg, *op. cit.*, p. 186.

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but such marriages, though pretty common, are viewed with a certain amount of disfavor." According to a footnote by Mr. I. H. Hutton, this qualification does not hold for the Sema Naga.¹¹

In each of our four areas, then, there is not merely paternal descent, but a supplementary factor, viz., marriage with members outside of one's generation, and it is the leveling influence of this determinant that produces the Omaha-Miwok features. When, however, we consider these forms of marriage, it is clear that they are themselves functions of paternal descent. It is in patrilineal tribes that a son inherits his father's widows; it is patrilineal tribes that would substitute for a woman her brother's daughter (Omaha, Miwok) or her father's sister (Omaha). What we are here confronting is merely a somewhat unexpected but by no means especially rare extension of a familiar principle. Tylor has familiarized us with the idea of primitive marriage as a compact of groups rather than of individuals. Individuals of the same group are reckoned equivalent: sisters from one household are exchanged for sisters of another, a deceased wife is superseded by her sister, a deceased husband by his brother without further payments, etc. We can easily understand that in small communities such substitutions might be difficult if one rigorously adhered to the principle of equivalence *within the generation*. We can also understand that the economic value of women might stamp them as the most valuable portion of a man's estate and that the heir is often bound to be of a lower generation. As soon as the notion is waived that social equivalence holds only within the generation the Omaha-Miwok Lango customs spring into being. *What* relatives shall be deemed equivalent will depend largely on whether there is a maternal or a paternal bias.

There is nothing in the nature of things that would inhibit such extensions of the ideas underlying the levirate and the sororate to matrilineal peoples. We might accordingly expect to discover corresponding phenomena among matrilineal tribes, and this is justified. Morgan found that a comparable cross-cousin nomenclature occurred among the Crow, Hidatsa, Choctaw, Creek, Cherokee, Pawnee, and Laguna; in other words, in the Northern and Southern Plains, the Southeast, and the Pueblo

¹¹ Mills, *op. cit.*, pp. 95, 155.

areas.¹² These tribes all possess one or all of the following traits: they call the father's sister's son by the same word as the father; the father's sister's daughter and her female descendants through females ad infinitum are all classed with the paternal aunt; the mother's brother's son is a son, and the mother's brother's daughter a daughter (more frequently for a male than a female speaker); the mother's mother's brother is equated with the maternal uncle. Rivers pointed out that Codrington had recorded a similar scheme in the Banks Islands (Melanesia);¹³ and our present knowledge of relevant American data has been succinctly summarized by Spier.¹⁴ From Spier's statement we learn that the features in question also occur on the Northwest Coast, among the Tlingit and Haida; and in California, among the Southern Pomo and Wappo.

Not one of the tribes listed is patrilineal; the majority have definitely maternal clan organizations; the Pawnee, Pomo, and Wappo are credited with a definite matrilineal bias. The Banks Islanders are organized into maternal clans. It is obvious that with maternal descent my father's sister's son and daughter are bound to be in my father's and my father's sister's clan; also that the female descendants of my paternal aunt through females will always be of her clan. It is equally clear that if descent were paternal these alignments would no longer hold. With two exogamous clans, e.g., if my father is of clan I, his sister is also I, but her husband necessarily belongs to II, and the father's sister's children are II. The agreement of the Crow cross-cousin scheme with matrilineal reckoning is thus perfect.

Again, we may take a test case. What happens if we eliminate the clan factor by taking the father's sister's son's daughter instead of the father's sister's daughter's daughter? The Crow are no less logical than the Omaha:

father's sister's son = father
(father's sister's son)'s daughter = father's daughter
father's daughter = sister

The daughter of the male cross-cousin defined above is thus not a paternal aunt, but a sister. In short, the Crow peculiarity is a function of matrilineal descent.

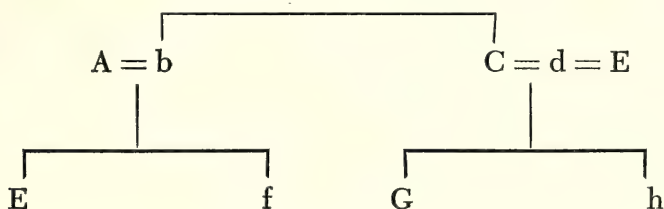
¹² Morgan, *op. cit.*, pp. 188, 191, 197, 262.

¹³ W. H. R. Rivers, *Kinship and Social Organisation* (London, 1914), pp. 28, 53.

¹⁴ Spier, *op. cit.*, p. 74.

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But here we are met by a difficulty exactly like that in the Omaha case. Not all matrilineal tribes have systems of the Crow type. Most of the Iroquois and the Tsimshian lack the traits characteristic of this type. What is the differential condition that makes for the Crow peculiarities? The Omaha-Miwok cases suggest looking for a special form of marriage that might produce the observed equations, and Rivers offers a solution in consonance with the Melanesian data. In the Banks Islands a man inherits his maternal uncle's wife. The result is plain.



From the point of view of G, the son of E's maternal uncle, E is his (G's) father's sister's son. Since this cross-cousin inherits d, who is G's own mother, E becomes G's father and is thus simultaneously his father and his father's sister's son. But inasmuch as E is now a father to G, f—being E's sister—is promoted to the status of father's sister to G.

In pure logic this explanation is unexceptionable. Empirically it is not adequate, for we cannot as yet demonstrate the inheritance of a man's widow by his uterine nephew except in Melanesia and on the Northwest Coast of America. The Crow and Hidatsa might conceivably be added because of a feature of their systems unique in North America: like the Melanesians of Leper's Island¹⁵ they lack a term for mother's brother, classing him as an elder brother—a classification once more intelligible on the theory that clan considerations may outweigh the principle of generations. Though we do not actually know that Crow and Hidatsa nephews inherited their maternal uncle's widows, such inheritance, as Dr. Lesser has pointed out¹⁶ would thus be a simple application of the levirate, which in the usual form is practised

¹⁵ A. B. Deacon, "The Regulation of Marriage in Ambrym," *RAI, Journal*, LVII (1927), 327.

¹⁶ Alexander Lesser, "Kinship Origins in the Light of Some Distributions," *American Anthropologist*, XXXI (1929), 720.

by both tribes. However, for the three remaining areas—the Southern Plains, the Southeast, and the Southwest—I have not been able to ascertain marriage with the uncle's wife or other unions with members of a higher generation. This may be due merely to defective observation, but that cannot be taken for granted. Hence, we must again cast about for alternative solutions.

At least one emerges from recently recorded Navajo data. The special peculiarity registered for the Crow, Hidatsa, and Hopi of merging the elder brother, maternal uncle, and mother's mother's brother terms¹⁷ would be explicable from the Navajo custom by which a man marries his wife's daughter by a previous marriage.¹⁸ In such a case, the child, g or H, borne by the daughter, f, has for a second "mother" his maternal grandmother, b, since she is his father's, A's, wife. Since the mother's mother = the mother, the mother's mother's brother = the mother's brother. Further, since

C = b's son and
also C = f's son, because she marries his father,
C = H's brother
Actually, C = H's mother's (mother's son)
C = H's mother's brother
Whence, mother's brother = brother

But from this the most distinctive features of the Crow type directly follow:

mother's brother's son = brother's son = (man speaking) son
Whence, by correlation, father's sister's son = father
father's sister's daughter = father's sister

In short, the Crow type is deducible from Navajo marriage arrangements as well as from nepotic widow-inheritance. Unfortunately we are not informed as to the distribution of the custom permitting simultaneous marriage with a woman and her daughter. The Navajo themselves, it must be admitted, do not explicitly carry out the logical implications of the arrangement.

¹⁷ R. H. Lowie, *Notes on the Social Organization and Customs of the Mandan, Hidatsa, and Crow Indians*, AMNH, Anthropological Papers, XXI (1917), 59.

¹⁸ G. A. Reichard, *Social Life of the Navajo Indians*, Columbia University, Contributions to Anthropology, VII (1928), 59, 62 ff.

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However, Dr. Reichard's genealogical data demonstrate a rather frequent shifting of generation status, individuals being sometimes raised or lowered two degrees. Thus, one of her subjects calls his father's mother's mother's brother's son his elder brother—ostensibly because the addressee and the speaker have fathers of the same clan who are accordingly rated as brothers.¹⁹ But with maternal descent every man is in the same clan as his mother's mother's brother, hence the stressing of clan alignment would favor identification of this relative with the elder brother, which actually occurs in explicit form among the Hopi, next-door neighbors of the Navajo. It seems far from improbable that the frequent departures of Navajo nomenclature from generation lines are correlated with their highly distinctive form of marriage.

Perhaps there are still other matrimonial regulations that might produce the Omaha or the Crow type of nomenclature. At all events, the general problem of these atypical systems may be formulated as follows. When a tribe practises the levirate and sororate or is organized into clans and recognizes the matrimonial equivalence of generation-fellows within a clan, the kinship terminology is likely to follow the Dakota-Iroquois type. As Dr. Lesser and Mr. Deacon have suggested independently, when equivalence is extended to members of higher or lower generations the Omaha or the Crow type evolves.²⁰ But which of the two? That will depend, I suggest, on the rule of descent, for the relatives who may be substituted, or who are added as fellow-spouses, are selected on that principle. We do not find a Navajo woman offering a father's sister or a brother's daughter to her husband as a supplementary wife; and correspondingly it is not the Omaha wife that makes of her daughter a fellow-spouse.

In some earlier publications I inclined to explain kinship terminology as far as possible from the clan only, to the virtual exclusion of specific marriage rules. I thus took issue with Rivers, and was opposed by Sapir, who stressed the levirate and sororate, and Gifford, who was impressed with the marriage of a man with the daughter of a wife's brother. Quite recently, Dr. Durlach has

¹⁹ *Ibid.*, p. 83.

²⁰ Lesser, *op. cit.*, p. 711. Deacon (*op. cit.*, p. 328) regards inheritance of a maternal uncle's widow as an extension of the levirate.

again taken me to task for assuming "too general" a point of view: "the reasons why certain terms should be extended beyond others, why some should override generations, and others not, must after all be sought in more specific causes, such as marriage regulations, social functions, or language."²¹ While I do not understand the last of these suggested factors, I have long been in sympathy with the remainder of the statement.²² What I insist upon is that the more specific matrimonial arrangements are themselves a function of the rule of descent. For example, in her recent publication Dr. Durlach has, so far as I know, been the first to register certain intergeneration forms of marriage. The Tlingit frequently married their elder brother's daughters, and a union with one's father's sister was preëminently proper.²³ These arrangements characteristically occur in a *matrilineal* society.

²¹ T. M. Durlach, *The Relationship Systems of the Tlingit, Haida and Tsimshian*, American Ethnological Society, Publications, XI (1928), 13.

²² R. H. Lowie, *Primitive Society* (New York, 1920), p. 37.

²³ Durlach, *op. cit.*, pp. 64 f.

The Family as a Social Unit

A FAMILY IS "THE GROUP OF PERSONS CONSISTING OF THE parents and their children, whether actually living together or not" (Murray's dictionary). The concept may be enlarged to embrace "those who are nearly connected by blood or affinity," but such expansion makes for greater vagueness. Adhering to the narrower definition, let us ask whether human society must a priori be constituted of family units. The answer is negative. There are sexually reproducing species without a semblance of family life, hence the segregation of husband, wife, and child into a distinct group remains to be empirically demonstrated. As a matter of fact, the existence of such a unit in early man has been categorically denied by many writers. In the beginning, we are told, was promiscuity—sexual license unchecked by any restraint. The earliest inhibitions prevented interbreeding of parent and child; they were followed by interdicts against the union of siblings (brother and sister); and so by a series of reformatory movements humanity finally attained the giddy heights of Victorian monogamy, at least in theory.

Unfortunately, we can know nothing directly about the sex life

Papers of the Michigan Academy of Science, Arts, and Letters, XVIII (1933), 53-69.

of man's immediate precursor, and a comparison of primate behavior, though definitely ruling out certain assumptions, offers a minimum of positive fact for the reconstruction of ancestral habits. A zoölogist, Mr. Gerrit S. Miller, Jr., has recently brought together what is known. He has proved that, contrary to a widespread misconception, virtually all the primates observed lack a rutting season. Accordingly, it is in the highest degree improbable that man's immediate forerunner mated seasonally. Like his fellow-primates, he was presumably ready to indulge in amours whenever an occasion arose. Furthermore, it seems that recent human aberrations have their counterparts among primates, and their potentiality may thus well be a heritage from a fairly remote past.¹

From available information, however, we can gather nothing to test the theory of early human promiscuity. Indeed, the field observations on the nearest anthropoids, chimpanzees and gorillas, are indecisive and at times contradictory as to the traits of the same species. Reichenow, for example, credits the gorilla with monogamous habits, while Akeley cautiously suggests the possibility of polygamy. "The truth is," he wisely adds, "that people know little about the habits of the gorilla."²

Yerkes, with exemplary restraint, makes the following statement:

Our tentative inference is that both monogamy and polygamy exist in one or another or all of the anthropoid types and that in all probability both relationships are discoverable in each of the manlike apes. With many misgivings we propose as order of increasing probability of monogamic relation: gibbon and siamang, gorilla, orang-utan, chimpanzee. Much more systematic, thorough, and critical investigation than has heretofore been conducted will be essential to discover the truth. Indicated as points of contrast among the three types of great ape are temporary monogamous or polygamous relations in the orang-utan, relatively permanent monogamous and

¹ G. S. Miller, "Some Elements of Sexual Behavior in Primates and Their Possible Influence on the Beginnings of Human Social Development," *Journal of Mammalogy*, IX (1928), 273-292; *idem*, "The Primate Basis of Human Sexual Behavior," *Quarterly Review of Biology*, VI (1931), 379-410. After writing this paper, I find that Miller's inferences are challenged by Dr. Solly Zuckerman in *Social Life of Monkeys and Apes* (New York, 1932). The matter is one for zoölogists to decide.

² Carl E. Akeley, *In Brightest Africa* (Garden City, 1925), p. 247.

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possibly also polygamous relations in the chimpanzee, and in the gorilla a patriarchal family, with polygamy presumably in the mountain species and monogamy, possibly, in the lowland species.³

If we know nothing more positive about existing species, any dogmatic conclusion as to the behavior of a hypothetical, extinct ancestral type seems rash indeed.

On one point, however, we can be certain. Whatever may have been the mating habits of this or that precursor of *Homo sapiens*, no believer in evolution can deny a stage of promiscuity somewhere along the line, that is, of promiscuity in the technical sense of socially unrestrained lust. Anthropologically, there is no "index of promiscuity," calculated by dividing the number of actual mates, regardless of kinship, by the number of physically possible ones. From this angle, it is a question of "all or nothing." Is carnal desire checked in some of its manifestations by the disapproval of the group? If it is, there is no promiscuity; otherwise, there is. Take the case of a male gorilla which Akeley found with three females. The point is not whether the male cohabited with all three females. It is rather this: Assuming two of them to be his daughters, would the attitude of other gorillas be one of indifference or not? The situation is not inconceivable even on the human level. A widespread tale of Great Basin and Western Plains Indians revolves about this very theme. The trickster by his wiles gains access to his own daughters. In the story, however, such behavior arouses intense moral condemnation. Now, I, for one, fail to find evidence of such a social consciousness in either Koehler's⁴ or other data from the infrahuman plane. If this interpretation holds, the anthropoids are promiscuous. On the other hand, no known group of *Homo sapiens* is indifferent to the sex behavior of its constituent members. Wherever evidence is adequate, matings are judged—outlawed, reprobated, condoned, accepted, or definitely sanctioned. The definitely sanctioned forms of mating may be termed "marriage," and from them evolves the family. Nowhere are fornication and marriage submerged in an undifferentiated category of animal-like "copulation."

³ Robert M. Yerkes and Ada W. Yerkes, *The Great Apes, a Study of Anthropoid Life* (New Haven and London, 1929), pp. 542 f.

⁴ Wolfgang Köhler, *The Mentality of Apes* (New York, 1925).

A chasm thus yawns between *Homo sapiens* and the chimpanzee or gorilla. At what stage of evolution, then, was the leap taken from unjudged to judged sex behavior? I do not know. I venture a guess that Neanderthal man showed some discrimination. I so conjecture because he demonstrably had a social tradition as to craftsmanship, and it thus seems probable to me that he had likewise evolved norms of social conduct. I refuse even to guess whether Heidelberg man, Eoanthropus, Peking man, and Pithecanthropus displayed equal fastidiousness. I am content to believe that, somewhere between the more remote anthropoid ancestor and the more immediate hominid ancestor whose descendants constitute geologically recent humanity, there was a stage of uncontrolled sexual license.

I am not sure whether I agree or disagree with Mr. Miller as to the distance of this stage. He offers the argument that living samples of men are specialized survivors and that many races have become extinct. Hence, he infers, "the search among these specialized existing peoples for a race or tribe living under social conditions that represent anything closely resembling an unmodified reflection of man's primitive mentality can have little chance of success."⁵ Here everything hinges on the meaning of the terms "man," "closely," "primitive mentality." I not merely admit but contend that Andamanese, Fuegians, Australians, and Chukchi tell us nothing definite about the mentality of Piltdown or Peking man. I emphatically insist that no one primitive group represents the first hominid's mentality in unmodified form. But if such highly specialized groups as Andamanese, Australians, and others, without exception exercise social control of sex life, then such control does not date back to yesterday nor, say, to 4000 B.C., but, presumably, to a period embracing the earliest samples of *Homo sapiens*, even though some of the races of this species are irrecoverably removed from direct observation.

Time does not permit detailed consideration of more than one recent human society. I shall select the Australians, whose anatomical inferiority and crudeness in the arts of life have made them a favorite starting point for speculative historians on the origins of the family, religion, and what not. Moreover, they

⁵ Miller, "The Primate Basis of Human Sexual Behavior," p. 400.

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have been credited with a form of sex life that might be viewed as intermediate between promiscuity and obligatory monogamy, viz., "group marriage." This institution has been defined as the non-preferential mating of a group of men with a group of women. It would not represent promiscuity, inasmuch as Australians would never tolerate unions of brothers and sisters. But anyone who favors the theory of promiscuity in Aurignacian or Mousterian times would naturally regard that mixture of polyandry and polygyny involved in group marriage as a step toward increasing control of mating. On the other hand, so long as a whole group of men mated indiscriminately with a group of women, the family would remain non-existent as a social unit.

In the interests of concreteness I shall base my statement on what Professor Radcliffe-Brown describes as the clearest available account of Australian conditions, Warner's report on the Murngin living west of the Gulf of Carpentaria.⁶ This picture I shall eke out with supplementary data on the Australians and shall then proceed to cull relevant data from the literature on other groups. The questions asked will include the following: Is there a form of marriage as distinguished from cohabitation? If so, what are the social relations of husband and wife? Of siblings? Of parent and child?

To begin with the Australians, no Murngin is free to mate with whom he pleases, and in marriage he is always expected to obtain the daughter of his maternal uncle. Failing such a one, a substitute of equivalent kinship status would be sought, for example, the daughter of a mother's male cousin. Potential spouses may be betrothed prenataally. To be sure, a man wants the maximum number of wives safely procurable, but they are never chosen at random. In order to make social intercourse possible for them at all, Australians always range individuals into kinship classes. So, even when the Murngin raid a hostile camp the kidnaped women are allotted to men standing to them in the socially approved relationship. Similarly, adultery is almost always with a cousin of the prescribed category. By a natural extension of these ideas, which rest on the social equivalence of siblings of the same sex, a brother

⁶ William Lloyd Warner, "Morphology and Functions of the Australian Murngin Type of Kinship," *American Anthropologist*, XXXII (1930), 207-256.

inherits his elder brother's widow, and often the several wives of a polygynous husband are sisters or quasi-sisters.

Unquestionably there are "wrong" marriages among the Murngin, as among ourselves. Yet within certain degrees prohibitions are absolute and, apparently, never flouted. In other cases strong disapproval is voiced: a man who would carry on an intrigue with his "sister's daughter"—actually perhaps his third cousin's daughter—would be compared to a dog, and the woman would be liable to a severe drubbing.

Moreover, within the range of licensed unions a distinct ideal may be noted. A husband may have several wives, but he ought not to seek amours with other women; and a wife is normally expected to content herself with a single mate, her husband. The social relations of spouses, furthermore, assume definite rights and duties. A wife gathers wild fruits and small game; the man supplies fish, turtle, porpoise, dugong. Sentimentally, common devotion to the children constitutes a bond; and even apart from that factor indications are not lacking of an attachment reminiscent of romantic love.

In all this there is not the faintest suggestion of either promiscuity or group marriage. The parental relationship is extended so that a woman's sister may help her suckle two babies; and, in general, a child looks to a maternal aunt for food and care. This, however, develops quite naturally from the practice of sororal polygyny. But, though the principle of sibling equivalence holds, the immediate family group is distinguished. Thus, a childless husband observes food taboos, which are lifted with paternity, "but the child must be his own, not that of a brother." (The term "own" in this context will be discussed later.) It is the father who determines the type of initiation for his son, passes on the right to certain dances, and teaches the ceremonial routine. In short, a man takes a differential interest in "his" children.

The Murngin thus recognize a family unit, but that does not mean that it is *our* family pattern. A contrast at once appears with regard to siblings. Brother and sister can never be on terms of easy familiarity. A brother never sleeps in the same camp with her, and neither may address the other. Associated with such taboos

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we find the attitude of mutual helpfulness that to us seems altogether intelligible. A brother will give presents to his sister for her son and husband. Two brothers coöperate in economic pursuits and have a sense of joint ownership of property. This naturally in a measure embraces wives, but with such qualifications as to exclude unchecked communism even between true brothers. No younger brother appropriates a sister-in-law without permission. The elder brother preemptively claims his maternal uncle's daughters. When he has thus acquired two wives, the younger man has a strong *moral* claim on the next oldest sister of the household, and her father may urge the husband to waive his legal prerogative. Even here there is thus definite customary law, not license. But a brother's attitude cannot be the same as ours in a society which makes him look to his older brother as the provider of a mate, either after or during his lifetime.

The family picture would be further modified by the taboo, universal in Australia, forbidding all social intercourse between a man and his mother-in-law. Yet, notwithstanding the lavish use of such kinship terms as "father," "brother," etc., to embrace fairly remote kinsfolk, the immediate family group is clearly separated from the rest of the community. A prospective husband tries, first of all, to marry his "own" mother's "own" brother's "own" daughter; and the uncle provokes resentment if he marries off a daughter to a remote nephew.

We have seen that a married man's social status depends on his having "own" children. This distinction between near and remote kin of the same category holds throughout. Remote "brothers" ambush and slay one another, or at least suspect one another as potential adulterers; but between true brothers there is implicit trust and unfailing devotion. So in periods of ceremonial license distant, not "own," brothers participate in the temporary exchange of wives. Thus, at every step we stumble on clear-cut evidence for the aboriginal feeling that relationship to the next of kin is a thing *sui generis*. The resulting family is a bilateral unit since, from the child's angle, relations are maintained with both parental sides.

The condition described by Warner is not unique, but typical

for the island continent. Malinowski's synthetic review⁷ of the earlier literature and Radcliffe-Brown's more recent summary⁸ leave no doubt on that point. Throughout Australia the nearest equivalent of our political unit, the state, is a localized "paternal lineage" or "horde" owning and exploiting in common a definite territory. Such a group embraces as a permanent core a number of brothers with their sons, sons' sons, and so forth. The women of the group normally come from another similarly constituted horde. Of the children the boys remain, acquiring from early childhood that intimate economic knowledge of the hereditary land which is a prerequisite to survival. The girls marry outside their horde, so that female children are only temporary constituents of the group into which they are born. Within this clearly defined horde, however, the aborigines recognize a lesser social unit, to wit, the individual family of parents and children. "The important function of the family," says Radcliffe-Brown, "is that it provides for the feeding and bringing up of the children. It is based on the co-operation of man and wife, the former providing the flesh food and the latter the vegetable food, so that quite apart from the question of children a man without a wife is in an unsatisfactory position since he has no one to supply him regularly with vegetable food, to provide his firewood, and so on. This economic aspect of the family is a most important one and it is partly this that explains Australian polygyny. I believe that in the minds of the natives themselves this aspect of marriage, i.e., its relation to subsistence, is of greatly more importance than the fact that man and wife are sexual partners. . . . sexual relations between a man and a woman do not constitute marriage in Australia any more than they do in our own society."

I believe that the picture our foremost authorities give of Australian conditions may be generalized for recent races of man. Twelve years ago I wrote: "The bilateral family is . . . an absolutely universal unit of human society."⁹ These are strong words, but I still regard them as essentially correct. In only one area of

⁷ Bronislaw Malinowski, *The Family among the Australian Aborigines, a Sociological Study* (London, 1913).

⁸ A. R. Radcliffe-Brown, *The Social Organization of Australian Tribes*, Oceania Monographs, No. 1 (Melbourne, 1931), esp. pp. 4, 6, 11 ff., 103, 107.

⁹ R. H. Lowie, *Primitive Society* (New York, 1920), p. 78.

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the world am I able to detect phenomena tending to qualify this view. In parts of Oceania, where adoption plays an extraordinary role, children are reported to divide their time more or less evenly between two homes, thus participating simultaneously in two family groups. I have recently taken cognizance of such facts, writing: "In this extreme form the custom [of adoption] inevitably modifies the principle of the universality of the individual family."¹⁰ Let us note in passing that the exceptions occur in highly sophisticated horticultural societies which cannot possibly be regarded as illustrating primeval usage; and that the exceptions rest on a custom which by definition is derivative.

The general prominence of the family cannot, of course, be demonstrated without passing in review one primitive society after another, which space does not permit. I should like, however, to point out one rather significant North American phenomenon. If we examine the kinship systems of the rudest peoples in North America, the purely hunting tribes devoid of complex political, social and ceremonial organization, we find that almost uniformly they distinguish in speech the immediate members of the family from the more remote kin. That is to say, while the Australians recognize the distinction in behavior, the simpler North American aborigines go so far as to express the sense of the difference in their vocabularies: a father is not only treated differently from an uncle, but is designated by a separate term; similarly, a brother is not included in the same term as a cousin; and so forth. The fact that the majority of non-horticultural tribes from the Arctic to northern California and Nevada fail to merge these relatives strongly suggests that the family unit is clearly recognized precisely on the lowest cultural level north of Mexico. It appears as though the family enjoyed undisputed ascendancy at a very early period, its significance being subsequently modified, though never abrogated, by other forms of organization. Thus, in Australia the partial equivalence of siblings of the same sex readily qualifies the character of the individual family, though its persistence is now demonstrated beyond cavil.

Terms for social units, such as "family," have misleading sug-

¹⁰ *Idem*, "Adoption, Primitive," in *Encyclopaedia of the Social Sciences*, I (1930), 459-460.

gestiveness; therefore I shall try to indicate the empirical range of the data properly coming under this head. Let me first explain that the biological family is not necessarily identical with its social equivalent. A clever writer has recently credited me with a belief in the social omnipresence of the *biological* family. She contrasts with this the saner view of Radcliffe-Brown, who, while taking the biological group as the chief point of reference in a treatment of social organization, "gives due weight to more complex developments characteristic of many primitive societies."¹¹ Actually, there is no conflict; what is particularly important, both Radcliffe-Brown and I emphatically warn against attaching too much weight to the biological aspect of the unit. "Bilateral" and "biological" are not synonymous terms. When an Australian speaks of his "own" father, he does not necessarily mean his begetter at all, but the adult male whom he preëminently associated from infancy with a certain emotional behavior, economic activities on behalf of the household, and so forth. Elsewhere I have pointed out, on Rivers' authority, that among the Toda of southern India polyandry often makes the determination of paternity very difficult. But the natives do not care at all about biological paternity: that husband who performs a certain rite during his wife's pregnancy becomes *legal* father of all children borne by the woman until another husband goes through the same ceremony. "Biological paternity is completely disregarded, for a man long dead is considered the father of a child provided no other man has performed the essential rite."¹² So, in some South African tribes a man claims as his own legal issue the offspring of a duly purchased wife, even if she has for years been living in adulterous union with a lover. What counts, then, is not the biological but the legal kinship. The omnipresence of the bilateral family, then, means this: Virtually everywhere a male, who is not necessarily the procreator, and a female, who is not necessarily the bearer, maintain preferential relations to a given child or number of children, thus forming a distinct unit within any major social group.

The fact that substitutions for biological parental relations are

¹¹ Margaret Mead, "Family, Primitive," *ibid.*, VI (1931), 65-67.

¹² Lowie, *Primitive Society*, p. 48.

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possible and even relatively frequent is precisely one of the most outstanding revelations which ethnography has to offer to her sister science, psychology. For it sweeps away once and for all the assumption of a paternal *instinct*. In its place we must recognize a much vaguer tendency of adult males to form an attachment to infants of their species.

Toda and Bantu indifference to the identity of the procreator suffices to mark off their conception of the family from that traditional in Western civilization. To these natives the insistence on recognizing as one's children only those duly begotten by oneself must appear as ludicrously irrelevant physiological pedantry. Appraisal of the children's status may rest on quite different considerations. Among Northwest Californian Indians the equivalent of the Occidental bastard is the boy whose father failed to pay the customary bride-price, for with that blot on his escutcheon he is never permitted to enter the men's club house.

Socially, however, the family pattern is only moderately altered by the lack of interest in physiological bonds. For, in the examples cited, one male simply supersedes another as the embodiment of the paternal principle. In other words, a social tie of our own parent-child relationship category remains. That category may be more definitely affected by a maternal clan organization. Where such an institution occurs, the bond with the father and his kin is still recognized, but all children are, *for certain purposes*, reckoned as of kin only with the mother and, specifically, bear the name of *her* clan, not their father's. In this way may be set up a series of sentiments, of legal rights and duties, that come to compete with the parental ties and even enter into open conflict with them. By so doing they also inevitably clash with the family as an autonomous social unit. This appears most clearly where the avunculate holds sway. There the maternal uncle usurps, according to our notions, many paternal functions, and, correlatively, it is his uterine nephews and nieces that often stand to him in a relationship *we* regard as filial. Thus, he, and not the father, may dispose of a girl's hand; he, and not the father, will give certain kinds of instruction to boys; and, though in some patrilineal African tribes, a man's son inherits his *father's* wives, barring only his own mother, certain matrilineal American

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and Melanesian groups permit a nephew to marry the widow of his mother's brother. To take a concrete case, a Dobu in Melanesia cannot bequeath his name, land, status, or fruit trees to his son; all of them are automatically inherited by a sister's son. A man may indeed teach his son what he knows of magical formulae; but to his uterine nephew he *must* convey such knowledge.¹³

Nevertheless, the sociological father is not abolished by avuncular customs. In the very region from which my last example is taken Professor Malinowski has demonstrated the depth of attachment linking father and son. The lurid and tragic conflict between paternal sentiment and avuncular duty has never been more vividly set forth.¹⁴

Another condition modifying the pattern of family life may be generalized under the head of "sex dichotomy," which manifests itself in many ways. Among the Australian Murngin we found the rather widespread custom of brother-sister avoidance, which at once precludes one of the most typical forms of family intimacy in our civilization. But we also saw that such usage does *not* snap the bond which links siblings together: brother and sister may not chat together, but they do aid each other, and the brother is keenly sensible of certain duties toward his sisters. Another type of dichotomy separates husband and wife. In many communities, for example, in South America and Oceania, spouses never eat together—an arrangement almost inconceivable to us. Yet the Banks Islanders of Melanesia go further. Among them virtually every adult male has bought his way into the men's club house, which is strictly tabooed to women, while the men not only lounge and work, but eat and sleep there, paying intermittent visits to their wives. Notwithstanding this institution, the family still holds together, so far as a husband exercises definite rights over his wife and is bound to her and the children by fixed duties.¹⁵ Generally, we may say that the universal sex dichotomy

¹³ Reo Fortune, *Sorcerers of Dobu: The Social Anthropology of the Dobu Islanders of the Western Pacific* (London, 1932), p. 15.

¹⁴ Bronislaw Malinowski, *Crime and Custom in Savage Society* (London and New York, 1926).

¹⁵ W. H. R. Rivers, *The History of Melanesian Society* (Cambridge, 1914), I, 60-143; R. H. Codrington, *The Melanesians: Studies in Their Anthropology and Folk-Lore* (Oxford, 1891), pp. 101 f.

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as to occupation is precisely a factor that fosters the family unit, for such division of labor, with its frequently correlated part-time separation as to companionship, obviously accrues to the advantage of the common household.

One other significant feature must be mentioned as modifying family relations. There may be segregation by age or status as well as by sex, or both forms of cleavage may be combined. Among the Masai of East Africa the bachelors occupy a separate hut, where they are joined by the young girls of the village with whom they consort apparently *ad libitum*. This is promiscuity in the popular but not in the scientific sense. For with meticulous care the Masai abstain from sex relations both with kinswomen and with their prospective wives, i.e., girls betrothed to them in infancy. And this once more accentuates the persistence of the family concept. For notwithstanding the license of the celibates' corral, it is definitely expected that every youth and maiden settle down in marriage after they have had their fling. Premarital freedom is followed by regular family life.¹⁶

In other areas, for example, in parts of Australia, only the boys are separated from the married couples. Usually this takes place after an initiation ceremony, sometimes at the age of seven. Relatively young boys are thus to some extent liberated from parental influence and subjected to the precept and example of somewhat older members of their own generation. In Samoa the unmarried are segregated from married folk in distinct male and female groups. The bachelors cultivate the soil, cook for the masters of the several households, and perform necessary communal tasks. The female counterpart embraces widows and wives of commoners as well as spinsters, and seems to have grown out of the custom of having companions of the same age groups and older chaperons sleep with a chief's favorite daughter.¹⁷ Again, among the Banks Islanders the men's club was divided into degrees, membership into each being acquired by purchase. Thus, there was a separation not merely of spouses, but of fathers and sons: normally

¹⁶ M. Merker, *Die Masai: Ethnographische Monographie eines ostafrikanischen Semitenvolkes* (Berlin, 1910), pp. 44, 84.

¹⁷ Margaret Mead, *Social Organization of Manua*, Bernice Pauahi Bishop Museum of Polynesian Ethnology and Natural History, Memoirs, Bull. 76 (1930), pp. 14, 92 f.

a man would eat neither with his wife nor with his children, and a mother would be dissociated from her sons as soon as they had entered the club house, an act which was rarely deferred until adolescence.

I have thus not merely admitted but stressed the diversity of family patterns in recent human societies. This differentiation, however, virtually never militates against the principle that husband, wife, and child constitute a definite social unit set off from other like and unlike units in their community.

Lest the oddity of some savage arrangements make us lose our sense of perspective, it is well to recall historic changes in the concept of the "family" as held by civilized peoples. Certainly the Chinese are not lacking in a family sense, but it is coupled with notions foreign to us of wifely duty, of polygyny, and of concubinage. Scriptural patriarchs, too, were polygynous and concupiscent, but no one challenges the prominence of the family in Biblical times. Much nonsense is lavished nowadays on the destruction of the family by industrial civilization. Yet the legal ties between parent and child, husband and wife, are clearly recognized. What has happened is an alteration of the family ideals among large portions of our population. For better or worse, the change from rural to urban residence, the stress of economic conditions, an individualistic ideology, the partial abandonment of traditional religious doctrines have jointly affected the relationships involved in the family concept. In the latter half of the eighteenth century Dr. Samuel Johnson, that paragon of Christian piety, laid it down as a principle that "wise married women don't trouble themselves about infidelity in their husbands." He considered a woman who should turn the tables on an erring husband as "very fit for a brothel." These ideas, I believe, are no longer universally held with equal fervor. What I should like to point out is that between the upholders of a double standard and the modern sex egalitarians the difference is roughly like the difference between either and the Murngin or Banks Islanders. Only those iconoclasts would fall outside the common practice who should consign infants to communal baby farming and who would not tolerate any but quite temporary sexual attachments. Such

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societies have indeed been reported with much extravagance of vituperation, but with great frugality of proof.

A few conclusions of general interest may be summarized:

1. We know nothing whatsoever about the sex behavior of the immediate forerunner of modern hominids except that it very probably conformed to the generalized primate norm. Specifically, if Mr. Miller's summary is trustworthy, this implies the lack of a rutting season.

2. Though we cannot picture the sexual life of the proto-hominid, we may be sure that there was a stage of promiscuity, i.e., of socially unchecked sex activity. For, by definition, social checks are a characteristic of culture; hence before there was a culture there was, in the scientific sense, promiscuity.

3. All the unequivocally rude tribes of the world—Andamanese, Bushmen, Australians, Fuegians, Paiute—have a violent reaction against incest with the closest kindred. It is, therefore, extremely probable that this sentiment is of great antiquity.

4. Nevertheless, I no longer believe, as I once did, that incest is *instinctively* objectionable to man. On the one hand, I am assured on good legal authority that the criminal calendar of Western nations shows relatively many instances of paternal lust directed against daughters; and if only one tenth of psychoanalytic evidence is rated valid, the Oedipus complex remains as a factor to be reckoned with. As regards siblings, we have at least three historic cases in which the supposed instinct was deliberately set aside—ancient Egypt, Peru, Hawaii. In each of these aristocratic societies no mate was considered more appropriate for a ruler than his own sister, the only one, evidently, who fully shared his illustrious pedigree.

The aversion to incest is, therefore, best regarded as a primeval cultural adaptation which certain individuals potentially or actually override in all societies and which certain sophisticated societies have expressly disregarded in the interests of an inflated sense of aristocratic lineage.

5. There is no parental instinct. No man can know instinctively that he is the begetter of an infant presented by his wife. Demonstrably, savage men in many and diverse societies utterly and

deliberately ignore the question of physiological relationship while emphasizing that of sociological kinship. The maternal sentiment seems to rest on a firmer basis. Actually, economic pressure or the desire to avoid the shame of an illegitimate birth may be stronger. Among the Murngin, "Sometimes a mother kills her newborn babe because it has followed too closely to her others and she has not enough milk to feed it." Here, as well as in many other savage communities, the superstitious objection to twins invariably leads to the immediate killing of at least one of them.

What is of course universal in the interests of group survival is a generic interest of adults in children. This sentiment, however, as we have just seen, is not manifested by all members of the species uniformly, but may be ignored by the superior force of utilitarian rationalism or ideological irrationalism.

6. Every known society distinguishes between mere cohabitation and that socially approved form of relatively permanent cohabitation known as marriage. It may not be superfluous to point out that, as there is social fatherhood without the notion of procreation, so there is frequently social wifehood without physiological relations. A man may inherit a woman so old that she is unfit or undesirable from a sexual point of view; nevertheless, she would engage in the feminine occupations with the other women of the household and would be entitled to protection and care on the part of its master. To cite a concrete case, among the Manyika of East Africa a woman becomes the property of her elder sister's eldest son. "He does not cohabit with her, but otherwise has complete control over her. He may keep her at his kraal, where she does the usual woman's work for him. She has no recognized husband, but is encouraged to have a lover or even several." The children from such unions, it is interesting to add, are in no way under the tutelage of their biological father, but are wards of the man who inherited their mother; he, and he alone, receives the girls' bride-price and provides the boys with the wherewithal for acquiring a wife.¹⁸

7. Apart from minor modifications or rare and highly localized deviations, the family based on marriage is a quite general phe-

¹⁸ Charles Bullock, *The Mashona, the Indigenous Natives of S. Rhodesia* (London, 1928), p. 65.

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nomenon in known samples of *Homo sapiens*. A man socially functioning as a father and husband practically everywhere combines with a woman functioning as mother and wife to provide for their common household and the children begotten by them or by legal fiction reckoned as their offspring. Since this pattern is common precisely to the unequivocally rudest known tribes, it is presumably one of great antiquity in *Homo sapiens*. How far back it goes in his history and to what extent it even antedates him, no one knows.

9

Nomenclature and Social Structure

NO ONE CAN DOUBT THAT THE LINGUISTIC APPROACH SHEDS light upon certain problems of social organization. My objection to using it as exclusively as has sometimes been done is that it does not shed enough light. As an introduction to my main argument, I can do no better than to quote specific data, as given below:

<i>Kwakiutl</i>	<i>Terms</i>	<i>Haisla</i>
father	ōmp (op)	father, father's brother, men of father's clan and generation
mother	abe'mp (abu'h)	mother, mother's sister, women of mother's clan and generation
maternal or paternal aunt	anē's (ani's)	father's sister, women of father's clan and generation
maternal or paternal uncle	qulē'	
_____	(xwatla'p) ¹	mother's brother, male clansman of parental generation

This paper was found among Professor Lowie's unpublished manuscripts. It was written after his retirement in 1950.

¹ Possibly a phonetic equivalent of the preceding term.

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The result is striking: Haisla nomenclature is not lineal, but "bifurcate merging," like that of so many tribes with a unilateral organization. But this is precisely what the Haisla have by virtue of their six matrilineal clans. In this respect they sharply depart from their southern congeners. According to Boas's latest exposition, these have nothing that can properly be called a clan. For despite the observed preference for the paternal line in the transmission of privileges, automatic patrilineal affiliation of all siblings and exogamy within the paternal line are lacking: the marriage, reported as orthodox, of a man with his younger brother's daughter would be anathema in any typical patrilineal clan system.²

In other words, that Kwakiutl tribe which has a clan system reflects clan affiliation in its kinship nomenclature; those Kwakiutl groups which lack a clan system very naturally do not manifest any such influence; the one term originally reported as embracing "group of supposed common descent" now stands revealed as very often applying to outsiders, to mere friends.³

These phenomena thus parallel those discovered among the Hopi of Arizona, a people differing from near-by fellow-Shoshoneans in the possession of both clans and bifurcation with merging.⁴ The case differs in that the clan system with its correlated features finds its maximum intensity of development within the Pueblo area among the aberrant Hopi, while the Haisla scheme is clearly but the pale reflection of a Tsimshian pattern. It is wholly probable that the clan idea and the associated terminology were borrowed in the lump from northern neighbors.

In this connection I should like to revert to a perennial moot-topic. How far does the nomenclature of relationship correspond to social structure and usage? I share Kroeber's and Gifford's revolt against the intransigent determinism of Rivers; I agree that there is a linguistic aspect to the problem that demands investigation.⁵ But I agree in the spirit in which a fair-minded

² Franz Boas, *Race, Language and Culture* (New York, 1940), pp. 360-367.

³ *Ibid.*, p. 368.

⁴ R. H. Lowie, *Hopi Kinship*, AMNH, Anthropological Papers, XXX (1929), 380.

⁵ See A. L. Kroeber, "Athabaskan Kin Term Systems," *American Anthropologist*, XXXIX (1937), 602; E. W. Gifford, "A Problem in Kinship Terminology," *ibid.*, XLII (1940), 190 ff.

biologist of the mechanistic persuasion acknowledges that he is unable to reduce life to purely physico-chemical formulae, yet insists that life cannot be *explained* in any other way. I contend that while we cannot, and probably never shall, explain all terminological features in social terms, it is a case of either such explanation or no explanation whatsoever.

For, deprecating as I do the extreme position assumed by Rivers, I cannot but share his perplexity as to what is meant by a linguistic determination or interpretation of kinship terms.⁶ When language subsumes under a common rubric, two phenomena, whether guinea-pigs and pigs or brothers and brothers-in-law, the original labeler evidently must have detected some resemblance between the phenomena he grouped together. But whatever was the common denominator between the guinea-pig and our porker, the association was evidently not inevitable since the Spanish equivalent is *conejillo de Indias*, "little Indian rabbit." Now what are the conceivable bases for linking relatives under one term? In his justly famous paper first introducing the linguistic point of view into this discussion,⁷ Kroeber listed eight categories, of which virtually all rest on social groupings. Specifically, in accounting for the fusion of the male cousin and brother-in-law concepts (Dakota woman speaking), he points out that these kinsmen are alike in sex and in being of the sex opposite to the speaker's, that they are of her own generation and not in her direct line of descent. But sex dichotomy, generation, collateral versus lineal descent are one and all sociological concepts. In this article, Kroeber differs from Morgan not in eschewing sociological interpretation, but in repudiating a specific type of sociological interpretation, viz., that which rests on forms of marriage.

The corrective value of Kroeber's and Gifford's several expositions against easy-going enthusiasm for short-cuts is very high, and the same must be said of Aginsky's and Opler's recent papers.⁸ From Aginsky we learn that a classification traditionally

⁶ W. H. R. Rivers, *Kinship and Social Organisation* (London, 1914), p. 9.

⁷ A. L. Kroeber, "Classificatory Systems of Relationship," *RAI, Journal*, XXXIX (1909), 77 ff.

⁸ B. W. Aginsky, "The Mechanics of Kinship," *American Anthropologist*, XXXVII (1935), 450; M. E. Opler, "Apache Data Concerning the Relation of Kinship Terminology to Social Classification," *ibid.*, XXXIX (1937), 201 ff.

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associated with special forms of marriage or descent may result from the speaker's adoption of either parent's identifications. Opler finds challenging disparities in nomenclature between Apache tribes sharing the same behavior towards a particular relative. In other words, Aginsky demonstrates that the same effects may spring from unlike causes; Opler, that identical causes seem to yield diverse effects. Nevertheless, we note with interest that these authors are very far from abandoning sociological causation on principle. Aginsky pertinently asks what affects the choice of one or the other parent's identifications and answers in terms of economics, residence, inheritance. Opler brilliantly suggests that terminology is not fully determined for the simple reason that a relative may be the focus of a whole series of behavior patterns, some of which may be stressed in one tribe, others by a related tribe. For instance, married Apache sisters are in many ways socially equivalent, and if this aspect of their functions happens to gain emphasis a single word will embrace both of them. But sororal polygyny being a possibility rather than a frequent phenomenon, there was an equal chance for stressing the differences between mother and maternal aunt.

It is clear, then, that in the light of our present insight a one hundred per cent correlation between a specific determinant and a terminological correlate cannot be admitted without the most rigorous proof. But it is not a case of "all or nothing" any more than in other scientific situations. Geographers are not humbugs when they tell us that temperature depends on distance from the equator even though snow covers the top of Kilimanjaro. Quite properly they adduce the "additional factor" of altitude to supplement that of latitude. I see no logical objection to following the same procedure in ethnology: it is the reality of the factors, not their number that seems significant to me.

I believe some difficulties disappear if we keep in mind how the problems of kinship terminology arise. Morgan found a terminology among the Seneca very different from ours and subsequently found great similarity between Seneca and Ojibwa and even between Seneca and Tamil. What underlies such distributional data? The question is a perfectly natural one and fairly clamors for an answer. The linguistic "interpretation" would come to this, that a fortuitous concatenation of psychological

causes unrelated to social structure and operating on the principle of free association by which some Indians called the horse a "deer" and others "a mysterious dog" made the Tamil in India and the Seneca of New York classify kinsfolk in a manner so similar that an analysis of the one is "nearly a literal transcript" of the other.⁹ On this hypothesis it seems odd that a distant Dravidian people should concoct a scheme so much closer to the Seneca pattern than that of many fellow-Americans, including even some fellow-Iroquois tribes! I must confess that my craving for an interpretation remains wholly unsatisfied by such a statement. Ridiculous as is Morgan's explanation in terms of racial affinity between the aborigines of India and of America, it goes at least through the forms of an explanation.

Now the supposition that similarities in remote areas are due to similar causes may be erroneous in the light of Aginsky's and Opler's considerations, not to mention Boas's and others' principle of convergence generally, but at least it does not leave us completely up in the air. If we single out a particular common factor as *the* cause, we probably overstate the case but are at least attempting a proximate analysis. I certainly did overstate the case in my earlier treatment of the subject when I blandly interpreted the overriding of the generation principle by exogamous grouping.¹⁰ But I still believe that I was wrong only in the sense in which a geographer is wrong who derives temperature wholly from latitude. Internal analysis of the Omaha system shows that when relatives otherwise indistinguishable as to proximity, generation, and sex differ in clan affiliation they are differently designated. The clan factor does not account for everything in view of the differences typified by the Crow, the Omaha, and the Dakota, but neither can it be ignored: we require "an additional factor" corresponding to our climatologist's altitude.¹¹

⁹ L. H. Morgan, *Systems of Consanguinity and Affinity* (Washington, 1871), p. 387.

¹⁰ R. H. Lowie, "Exogamy and Classificatory Systems of Relationship," *American Anthropology*, XVII (1915), 238 (reprinted as paper No. 2 in this volume).

¹¹ *Idem*, "The Omaha and Crow Kinship Terminologies," *Verhandlungen des XXIV. International Amerikanisten-Kongresses*, pp. 102 ff. (reprinted as No. 7 in the present volume).

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Mr. Gifford's argument against the sociological interpretation of the Omaha system is wholly convincing if (a) that system is derived automatically from patrilineal descent; (b) only the California data are envisaged. For on the former assumption not only the Southern Shoshoneans and Pima (whom he cites), but the Ojibwa, the Maricopa, the Baganda, the Jagga, the Murngin, the Tungus, ought all to share the Omaha nomenclature. And with a restriction to California it would be quite arbitrary to pick out the six tribes with patrilineal descent, regarding them as the originators of the terminology and the ten clanless tribes as the recipients.

This, however, does not do justice to the problem as it actually arose. What haunted me was the fact that here was a nomenclature utterly distinct from that of fellow-Siouans, who have at least two different modes of classification, yet basically like that of a solid block of neighboring Algonkians; and these again differed from such fellow-Algonkians as the Ojibwa. And what obsessed me still more was the amazing fact that nothing like this reappears westward of the Southern Siouans until we get to central California! *That* is the cardinal problem. In broader perspective the contrary California instances do not weigh against the favorable ones in the ratio of 10:6. Rather, all the Californian instances represent one distributional block, the Southern Siouans and their Central Algonkian neighbors another, such tribes as the Assamese Lhota a third. Quite obviously the mere factor of patrilineal descent is inadequate; but that rule *plus* a specific form of marriage congruous with it does explain the observed distributional data and inclines the balance in favor of diffusion from the patrilineal to the clanless Californians in question, a result quite properly inadmissible on California evidence only.

The conclusion to which I am once more drawn is that a preliminary to sound inference is a rough knowledge of the total world or at least continental range of distribution. This naturally and inevitably yields certain problems. When these have been formulated, a more refined approach should set in with the comparison of groups linguistically and culturally most closely related. By concentrating on the points of difference we can then venture opinions as to which differences are functionally

related—in the correct mathematical sense of the term which, *pace* Dr. Blumenthal,¹² does not mean the same thing as causally related.

¹² Albert Blumenthal, "A New Definition of Culture," *American Anthropologist*, XLII (1940), 576.

PART II *Literature, Language, and Aesthetics*

"A Note on Aesthetics" (No. 10) and "A Case of Bilingualism" (No. 12) are reproduced here in part because they were esteemed by Lowie himself. He would probably have also included in this section chapter v, "Literature," of his book *The Crow Indians*. Since the decision was made not to make excerpts of Lowie's books in this collection of papers, that chapter has been omitted. In its place, although by no means its equivalent, two other papers have been selected: "Observations on the Literary Style of the Crow Indians" (No. 13) and "Some Cases of Repeated Reproduction" (No. 11). The latter is part of his *Studies in Plains Indian Folklore* that Lowie would also have wished reprinted. From both No. 11 and No. 13 the Crow texts contained in the original papers have been deleted. If any excuse other than expediency must be offered for this excision, it is that Lowie's Crow linguistic materials have recently been published by the University of California Press (*Crow Texts*, 1960, and *Crow Word Lists: Crow-English and English-Crow Vocabularies*, 1960).

A challenging article, "Native Languages as Ethnographic Tools" (*American Anthropologist*, XLII [1940], 81-89), properly belongs in this series of papers but has been omitted in view of its availability. The final article of Part II, the hitherto unpublished "Evolution and Diffusion in the Field of Language" (No. 14), might have just as legitimately been included in Part IV, "Theories and Theorists," where Lowie's views on genuine and spurious concepts of evolution emerge more fully.

A Note on Aesthetics

WHILE ATTEMPTING TO DETERMINE THE ARTISTIC STYLE OF Crow parfleches as compared with that of other Plains tribes, I hit upon the notion that it might be desirable to apply some of the methods in vogue in experimental aesthetics. Circumstances prevented me from carrying these inquiries very far. Nevertheless, I feel it may be worth while to record my measurements in the hope that they may stimulate others to make corresponding observations on a larger scale and particularly to undertake relevant investigations in the field.

Gustav Theodor Fechner, the founder of aesthetics as a branch of exact psychology, endeavored to determine what forms of a particular geometrical category were deemed most pleasing. For this purpose he employed three methods,—that of having his subjects choose from a series of, say, rectangles the most aesthetic samples; that of having them construct the desired forms; and that of noting objectively what forms predominated in actual use. Since the decoration of parfleches consists for by far the greatest number of instances of simple geometrical figures, it seems to present an excellent opportunity for applying Fechner's prin-

American Anthropologist, XXIII (April-June, 1921), 170-174.

ciples, though in the study of museum material the first two of his methods are of course excluded.¹

Inquiries of this sort have an ethnographic no less than a psychological interest. A priori it is indeed possible to assume that in respect of the simpler geometrical figures a single aesthetic norm is common to all mankind,—say, the principle of the “golden cut” examined by Fechner, according to which the ideal rectangle has sides bearing to each other the ratio of $1 \pm \sqrt{5}$ to 2, the lesser having a length approximately 61.8 per cent of the greater. But it is far more reasonable to expect certain differences in the aesthetic canons accepted in different regions. And if this anticipation were verified, we should have an additional set of features for differentiating cultures. What is more, by pursuing such studies it becomes possible to define existing differences with greater nicety: instead of contenting ourselves with the remark that one region favors an angular and the other a curvilinear style of decoration we may succeed in determining objectively that one tribe prefers a rectangle of one type, a neighboring tribe a rectangle of another type.

But the matter is not quite so simple as this formulation might suggest. After one has handled a fairly large number of specimens from a single group it becomes clear that the preferences are not clear-cut and absolute. For example, we cannot say that the Crow use, say, isosceles triangles for the simple reason that even the same bag may be painted with right-angled as well as isosceles triangles; and the latter again may vary enormously in their aesthetic character according to the angle enclosed by the equal sides. It appears that the aesthetic value of a simple form is affected by its position in the decorative field: what is proper in a marginal area may be taboo in the middle, and so forth.

In order to avoid the pitfalls just hinted at I decided to compare the parfleches of the Shoshoni with those of the Crow as regards a single figure in the same position, to wit, the rectangle in the center of the decorative area. The central rectangle has been rightly noted as a trait characteristic of the Shoshoni parfleche, though it is by no means found on all Shoshoni speci-

¹ Those interested are referred to G. T. Fechner, *Vorschule der Aesthetik* (Leipzig, 1876) and Ch. Lalo, *L'Esthétique expérimentale* (Paris, 1908).

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mens.² This feature is to some extent shared by the Crow. That it has a single origin historically cannot be doubted considering the geographical position of the tribes concerned and the lack of this motive on the parfleches of most other tribes. The question, then, is whether the borrowing tribe has transmuted the borrowed feature in consonance with its own aesthetic predilections and wherein such modifications consist.

So far as I know, the two flaps of a parfleche invariably bear the same ornamentation and it is plausible to assume that they are meant to be identical. But whatever may be the artist's ideal, she frequently departs from it as regards the dimensions of her figures. In some instances, indeed, the discrepancies proved decidedly startling. I also found that the parallel lines of the same rectangle were not always equal in length but sometimes varied in appreciable measure. Accordingly, in establishing my ratios I measured all the sides and averaged those determining the same dimension. Since in the majority of cases there is a frame round the central figure, this provided an additional rectangle for each flap, so that the number of ratios for any one parfleche is usually four. The shrinking of the rawhide and the partial obliteration of some of the lines make exact measurement difficult in some of the specimens, but of course the minor inaccuracies due to these causes are negligible for present purposes. Only in one case were certain lines so completely effaced that measurement was impossible.

In the table (p. 140) the fractions designate the specimens as registered in the catalogues of the American Museum of Natural History. The absolute measurements are given in millimeters, those relating to the parallel sides of the same rectangle being paired.

It would of course be vain to draw any far-reaching conclusions from the small number of cases available for comparison. If I venture to broach the subject, it is because it provides a valuable method for field-workers, which I hope they will not neglect. It is not always practicable to purchase large series of museum specimens, but few natives would object to having the figures on

² A. L. Kroeber, *Ethnology of the Gros Ventre*, AMNH, Anthropological Papers, I (1908), 172.

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Specimen	Shoshoni		Ratios
	Width	Length	
$\frac{50.1}{883}$	97, 98 88, 89	138, 139 136, 138	69.3 65.4
$\frac{50.1}{1166}$	202, 197 172, 160 210, 200 172, 183	303, 300 269, 270 299, 295 263, 267	66.2 61.5 69.0 66.8
$\frac{50}{2299}$	160, 159 175, 170 131, 130 148, 140	263, 265 282, 294 231, 225 250, 250	60.6 60.1 57.0 57.6
$\frac{50}{2346}$	161, 150 124, 114 153, 145 95, 100	210, 205 183, 176 191, 196 160, 161	74.9 67.0 67.2 60.6
$\frac{50}{2300}$	154, 154 133, 136 175, 175 145, 145	246, 244 222, 220 283, 290 266, 259	62.8 61.1 60.9 55.3
$\frac{50}{1179}$	175, 173 183, 180	208, 204 190, 200	84.4 93.3

Crow

Specimen	Width	Length	Ratios
$\frac{50}{6855}$	147, 143 102, 97 149, 148 111, 108	245, 249 206, 207 222, 221 180, 182	58.7 48.3 67.1 60.7
$\frac{50}{1774}$	130, 120 97, 102 120, 133 97, 105	145, 149 117, 114 157, 157 121, 125	85.0 86.2 80.2 82.1
$\frac{50}{6846}$	112, 124 90, 80 121, 113 86, 78	224, 228 190, 189 217, 223 177, 181	52.2 44.7 53.2 45.8

A Note on Aesthetics

Specimen	Crow—Continued		Ratios
	Width	Length	
*	8	12	66.6
	8	14	57.1
	7	9	77.7
	6	11	54.5
*	10	17	58.8
	9	17	52.9
	8	13	61.4
	8	15	53.3

* This specimen was photographed in the field and the proportions calculated from the negatives. Owing to the small size of the measurements obtained, differences between parallel lines are ignored here.

their rawhide bags (or other objects bearing designs) measured by an ethnological visitor. I certainly feel confident that had I been alive to this mode of research at the proper time I could have secured an imposing array of data on Crow parfleches that would have definitely decided the closeness of their kinship with those of the Shoshoni.

I will assume that the samples of ratios supplied by my two small series are typical and will collate the data in a table of distribution, uniting percentages by fives.

Ratios	Crow	Shoshoni
40-45	1	
45-50	2	
50-55	5	
55-60	3	3
60-65	2	7
65-70	2	6
70-75	0	1
75-80	1	1
80-85	3	1
85-90	1	0
90-95		1

The fact that the number of Crow cases above 80 is twice that of the Shoshoni is readily explained when we remember that the Crow piece in question has a framed rectangle while the

excessively broad Shoshoni parfleche is frameless. Few as are the ratios, all the data consistently point in one direction,—a preference of the Shoshoni for relatively wide rectangles in the central position. The narrowest Crow rectangle is much narrower than the narrowest Shoshoni one; the broadest Shoshoni rectangle is broader than the broadest Crow rectangle; the Shoshoni prefer quite decidedly the ratio of from 60 to 70, the Crow the ratio of from 50 to 60. On the basis of these figures the Shoshoni norm would fall somewhat above and the Crow norm somewhat below Fechner's ideal rectangle.

I have already indicated that I attach to these findings a merely tentative and suggestive value. Of course comparison should not be restricted to rectangles in a particular position but must be extended to other forms, say, the diamonds or hour-glass figures that are so prominent in the rawhide decoration of Plains Indians. A comprehensive inquiry of this sort is bound to yield interesting results for it will be as important to ascertain that there is practical unity of aesthetic reaction to geometrical forms as to determine tribal differences.

II

Some Cases of Repeated Reproduction

THE WATER FETCHER'S COMMUNICATION

IN 1910 AND 1911 I WITNESSED INITIATIONS INTO THE tobacco society of the Crow Indians. At one stage of the performance a man with a creditable war record was sent for water. After ceremonial preparations he dashed off, filled a vessel, and returned, thereupon reporting in a very low tone of voice to the owner of the initiation lodge.

Gray-bull, who had repeatedly served in this capacity, three times dictated to me the tenor of the water fetcher's (ak'ĩcde) communication.¹ Unfortunately I am unable to give the intervals between successive recitations. Nevertheless the variants present some points of interest. [Crow texts of the following translations are omitted, and their accompanying footnotes.]

TRANSLATIONS—(a)

1. On a war party they went, among them I went. 2. The people [person?] toward [post-position] they ran, they killed, his gun I

Studies in Plains Indian Folklore, Univ. Calif. Publ. Amer. Arch. and Ethn. XL (1942), 19–28.

¹ R. H. Lowie, *The Tobacco Society of the Crow Indians*, AMNH, Anthropological Papers, XXI (1919), 153 f., 185. I have corrected the orthography in the present paper and to some extent the translation.

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took. 3. Then I came back. 4. The tobacco you [plural] had planted when I reached, it was abundant extremely; round about the chokecherries were abundant extremely. 5. Then I came, the camp when I reached sick people there were none. 6. Peacefully the tobacco you were harvesting.

(b)

1. Chokecherries ripening [i.e., the summer] safely we shall reach. 2. Sickness there is none. 3. I went on a war party, someone was killed, a coup I struck. 4. My heart being good [i.e., happy] I arrived. 5. The tobacco you planted I saw, the tobacco was plentiful. 6. The tobacco to see I wished, I came, the tobacco was growing excellently. 7. The Crow well were faring, chokecherries in safety you were eating.

(c)

1. I went on the warpath, in safety [i.e., without loss] they killed someone, his gun I took. 2. Then I came, your gardens when I saw, the chokecherries were plentiful. 3. Round about buffalo were plentiful. 4. Then I came, when I came (?) and the camp I reached, toward camp I signaled. 5. When I came and the camp I reached, the Crow well were faring.

Evidently the form of the report is not fixed, for the same informant's versions reveal variations. But the substance is identical, being composed of three themes: the speaker's war experience; his inspection of the tobacco garden and its environment as he returns; and his auspicious findings. The emphasis is throughout on the rosy side of life: a successful raid; a plentiful crop of the weed believed to ensure the tribal welfare; abundance of food generally; and the prosperity of the people as foreshadowed by the inspection. What, then, is the nature of the discrepancies?

Apart from purely verbal differences, the initial cue "war raid" would present a seasoned brave, such as Gray-bull, with a number of possibilities, narrowed only by the need for eliminating untoward events. Of the four conventional feats of bravery recognized by the Crow, versions (a) and (c) introduce the capture of a gun, the other variant the striking of a coup. Since Gray-bull himself had in addition successfully led raids and cut horses from their pickets, the omission of these feats is not due to personal reasons. The failure to mention horses, however, may be due to archaizing, since some origin myths made the first

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tobacco planting preëquestrian.² Quite naturally a lucky raid includes a killing, but characteristically without loss of a Crow. This concomitant is understood in the first two versions, made explicit by the term *i'tsikya'ta* in the last. The "signaling to the camp" would be out of place in the second version, but would be as appropriate in the first as in the third, since *acis'-buxu'cuk* seems to imply signaling that the homecomers are bringing booty.

Since wild vegetable fare was of subordinate importance, the mention of buffalo in a single variant contrasts sharply with the appearance of cherries in each version—twice in version (*b*). However, this otherwise curious fact is readily explained: The season for the tobacco harvest is "when the cherries are ripe"; the phrase *ba'tsuə o'ce* is a cliché for designating the summertime; and like other standardized designations of seasons it appears prominently in prayers for long life and happiness.

The word *i'tsikya'ta*, already discussed in a special setting, is another cliché. Though requiring different translation according to the context, it invariably denotes a satisfactory situation, being simply the adjective *i'tsi*, good, with an adjectival suffix commonly denoting diminutiveness or affection. Thus, in a prayer the suppliant says, "*i'tsikya'ta baku' wiawak*," In safety I want to return; and elsewhere we find, "*i'tsikya'ta bawara'pbic bi'awuk*," Safely we'll take revenge (compare above).

The three versions are of some interest as illustrating in miniature the type of changes to be expected when the same individual reproduces, untrammelled by the necessity of letter-perfect repetition, the essence of a traditional pattern. Evidently the informant clings to some stereotyped expressions, but he has the choice of explicitly stating or merely implying some circumstance; he may amplify by adding a specific image (such as the abundant buffalo, or the peaceful eating of cherries, or the signaling), he may choose between symbols (coup or gun-capture).

A BATTLE WITH THE DAKOTA

The foregoing comparison was prompted by Dr. Lindgren's summary of Professor Bartlett's experiments on white subjects, each

² *Ibid.*, pp. 186, 188.

of whom reproduced after fifteen minutes, and subsequently at various intervals, a North American Indian tale he had read.³ It has been recognized for some time that individual storytellers within a tribe vary appreciably in their rendering of a tale; and this fact evidently affects intertribal comparison. But it seems equally important to ascertain how far the same individual departs from the norm as received by him. This would affect not only the evaluation of folk tales, but of "historical" traditions as well.

A good illustration is available. In 1910 I bought a sacred shield belonging to Yellow-brow, then in the prime of life, and his father. Several years later Yellow-brow gave an account of its history, terminating in the description of a battle with the Dakota.⁴ In 1931—without the slightest reference to this shield—the same informant launched into a long tradition, which similarly culminated in an account of hostilities, but with the *Cheyenne*. The native text, except for one prayer, has remained unpublished, but significant passages connected with the battle have been presented in English.⁵

Both times the narrator doubtless tried to picture the same occurrence. For one thing, the warriors prominent in both accounts almost entirely coincide in name; for example, Wants-to-die, Plays-with-his-face, Double-face, Young-white-buffalo, Passes-women. Secondly, both descriptions feature the nervousness of Double-face before battle, with some identical details, such as his desire to cry and to sing both sacred songs and those of the Big-dog society. Yet the enemy in the fuller report is the Cheyenne, not the Dakota tribe; the entire context differs; and along with amazing identities there are notable discrepancies not due to mere absence of items in the shorter narrative, which as a matter of fact embodies highly characteristic traits lacking in its rival.

I suggest the following explanation. Asked for the history of

³ F. C. Bartlett, M. Ginsberg, E. J. Lindgren, R. H. Thouless, eds., *The Study of Society, Methods and Problems* (London, 1939), pp. 363 ff.

⁴ R. H. Lowie, *The Religion of the Crow Indians*, AMNH, Anthropological Papers, XXV (1922), 415–418.

⁵ *Idem*, *The Crow Indians* (New York, 1935), pp. 230–236, 332–334; *idem*, "Crow Prayers," *American Anthropologist*, XXXV (1933), 440 ff.

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the shield he had sold me, Yellow-brow was primarily concerned with its validation as an object possessing supernatural power. Accordingly, he set out in characteristic Crow fashion, deriving it from a revelation in a vision. The visionary, after himself profiting from the shield, made replicas for his three sons and one nephew, bidding them above all to protect the women and children in camp rather than go on raids. Now, in his later version the battle follows the destruction of Dangling-foot and his small body of Crow by the Cheyenne, whence the thirst for revenge on the part of the survivors. Another good informant, Grandmother's-knife, also ascribed this massacre to the Cheyenne.⁶ But in the earlier of the Yellow-brow accounts Dangling-foot is not mentioned; there is merely a generic reference to the enemy's having destroyed a detached company of Crow. Inasmuch as "enemy" was almost coterminous with "Dakota," the informant naturally glides from the generic term into this specific description, adhering to the identification throughout. Even his conclusion, once more summing up the case for the value of his shield, is that ever since its acquisition the *Dakota* were repelled by the Crow.

This seems to explain very simply the shift from Cheyenne to Dakota, or vice versa.

Further, once launched on his glorification of the shield, Yellow-brow naturally enhanced its dignity by linking it with a stirring tradition of victory that probably had an original connection with quite different circumstances. However, when simply narrating what he regarded as the most striking events in the past of the Crow, he enlarged, indeed, on the battle, but the shield, however important to him, did not loom in his memory in that context, presumably because it did not really belong there.

This seems a factor to be reckoned with. A mind replete with the traditional lore often had alternative sequels for the same stage in a story. The lore, for example, may harbor two ways of escaping from a pursuing ogre; and a narrator conversant with both may choose one or the other according to individual preference or momentary caprice. That this is not pure guess-

⁶ R. H. Lowie, *Myths and Traditions of the Crow Indians*, AMNH, Anthropological Papers, XXV (1918), 185.

work is shown by an experience with Yellow-brow when he told the Old Woman's Grandson story. He actually told one episode in the briefer of two mutually contradictory forms current among his people, then corrected himself, retracing his steps so as to bring in a prerequisite element for the longer version.⁷

THE SLEEP-INDUCING FORMULAE

Stylistically the sleep-inducing formulae used by Grandson (p. 4), and according to some raconteurs also by the snakes he visits, are of great interest. The two Hidatsa and my four published Crow variants may be supplemented by the following Crow versions: an unpublished anonymous Grandson text dictated to me probably in 1916; an unpublished Grandson text dictated by Plenty-hawk in 1931, which differs appreciably from the printed version he had told me before; the three forms of the episode introduced, respectively, into the Twin cycle by Plenty-hawk and Gray-bull, and into the Old Man Coyote cycle by Grandmother's-knife.⁸ The total number of comparable formulae is thus increased to eleven, which follow.

PLENTY-HAWK VERSIONS

(a) 1931 Grandson Text

1. The rain is dripping fast; we sleep well, don't we?
2. At the river when we pitch camp, we hear the cicadas (?) calling and doze off, don't we?
3. When it's windy and the tent flaps flap together, we sleep well, don't we?
4. When it is windy and the pine needles rustle, we sleep well, don't we?

(b) 1914 (?) Grandson Myth

1. Whenever they moved camp by the riverside where there was plenty of shade, people would go swimming and in the shade they could not help but sleep.
2. On windy days they came to the tipis and heard the wind blowing. Then they would cover up with blankets and could not help but sleep.
3. When a big crowd of people moved toward the mountainside,

⁷ *Idem*, *The Crow Indians*, p. 109.

⁸ *Idem*, *Myths*, pp. 36, 81, 93; cf. with occurrences in the Grandson tale, pp. 56, 62, 72.

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they would hear a rustling in the pine trees. Then they could not help but sleep.

4. Late in the fall there are long rainy days. We would lie inside and put blankets over us and hear the rain strike the tipis, then we could not help sleeping.

(c) 1914 (?) Twin Myth

1. When a big crowd of people move and reach a river, they are always eager to get there. When they arrive, there will be a big shade and the river will be high. We'll smell the river and see the trees and the leaves floating down and the blackbirds singing over the river. After all have camped, everybody will go in for a swim and, sitting down afterward in the nice shade, they will fall asleep.

2. In the fall when the leaves have all turned yellow and are falling off, there are sometimes rainy days. They will be out somewhere far along in the evening and get wet, and when they get home they will take a blanket and cover themselves. When they have lain thus for a while, they can't help falling asleep.

3. Late in the fall when the days are windy, they will be out and come back home and lie inside. They will hear the wind blowing, then they can't help falling asleep.

4. When they move to the mountains and camp near the pines and the wind strikes the trees, they can hear the rustling in the pines and can't help falling asleep.

GRANDMOTHER'S-KNIFE VERSIONS

(a) 1916 Old Man Coyote Tale

1. When we go along and take a rest under the tall grass and the wind moves, we almost fall asleep; and when we do sleep, it's fine, isn't it?

2. It's fine when we are by the riverbank with ripples, isn't it?

3. Right in the mountains where the streams come out, it's nice to lie under the pines and hear the wind blowing through the needles and the water running. It is fine, enough to put a man into a dead sleep, isn't it?

4. When the day is cloudy, the thunder makes a low rumble and the rain patters against the lodge, then it's fine and nice to sleep, isn't it?

(b) 1916 (?) Grandson Myth

1. In the fall when it rains, we can hear the rain on the tipi, and we shall sleep well.

2. When we sleep among the pines with the wind blowing and we hear the sound of the pines, we sleep well.

Literature, Language, and Aesthetics

SCRATCHES-FACE VERSION

1. In the spring when cherry and plum blossoms are in bloom, when we kill a deer we cook it on the sunny side of a cherry-tree thicket. In the fall when it is cool we are out a long time and when we come back to our tipi and find it warm we go to sleep right away. Do we?

2. When out hunting in the mountains, when we have killed buffalo or deer toward evening and build a fire and cook, while we are cooking it grows dark. We are very tired. We take our cooked food and eat it. Rain comes and when we lie down to sleep, we sleep right away.

ANONYMOUS VERSION 1916 (?)

1. In the spring in the daytime when there is a little breeze we are wont to sleep well.

2. In the summer when the raindrops rattle on the tent we are wont to sleep well.

YELLOW-BROW VERSION

1. In the fall whenever there is a little wind, when we lie in some shelter, when dried weeds rub against each other and we listen, we generally get drowsy, is it not so?

2. In the daytime when it drizzles and the rain strikes the lodge pattering, we remain lying on the side, and warming our soles, then we fall asleep, is it not so?

3. At night when we are about to lie down, listening to the wind rustling through the bleached trees, we do not know how we get to sleep, but we fall asleep.

4. Having sought a hollow among the thickish pines, we make a fresh camp there. The wind blows on us, and we, rather tired, lie down and at the same time keep listening to the rustling pines, until we fall asleep.

GRAY-BULL VERSION

1. When we move early and camp late in the evening, we usually fall asleep and sleep soundly.

2. On windy days when we do not move and lie in the tipi we sleep soundly.

3. When after being out on rainy days all day we come back in the evening and sit by the warm fire, we sleep for a long time.

4. When we have been out from early in the morning, hunting and butchering buffalo till evening, we are tired and go to sleep right away.

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BEAR'S-ARM (HIDATSA) VERSION

1. There is a high butte. You can hear the wind rustling over the butte. Then in a moment the rustling ceases as if the butte had fallen asleep.
2. You hear brooks of water making a lapping sound. In a moment the lapping stops as if the water had gone to sleep.
3. You hear the wind blowing, blowing, then all of a sudden it dies down just as if it had gone off to sleep.
4. You hear the leaves of the trees rustling and flapping, and all of a sudden you hear no sound any more, just as if those trees were all fallen asleep.⁹

WOLF-CHIEF (HIDATSA) VERSION

1. The first stars you see are still, but toward daylight they always shake. When the shaking stops, they are asleep.
2. When the wind passes bluffs at night, it makes a sound but toward daylight it is still, the hills are sleeping.
3. When the wind passes the Missouri timber, it makes a noise; toward daylight it always stops, the timber is asleep.
4. At night the Missouri River makes a noise; at daylight it stops and sleeps.

These are evidently variations of a single theme with a single historical origin. We are confronted with a verbal form of the urge to play which Professor Boas has repeatedly stressed in other fields of art. It is clear that there are no definite limits to variability within the same general frame. For that reason a clear-cut classification is not easy, apart from the fact that in the Hidatsa tales it is the natural phenomena—hills, water, wind, trees, stars, Missouri—that fall asleep, not, as in all Crow parallels, human beings. A further difficulty lies in the fact that only three of the Crow versions are available in the original, so that the exact equivalence of certain expressions is uncertain. Nevertheless several points of interest emerge.

For one thing, eight of the eleven parallels, including both Hidatsa forms, are broken up into *four* sleep-evoking statements. It seems highly probable, therefore, that this represents the norm, as one might expect from the sacred number of these tribes.

Secondly, particular informants did not feel bound to adhere

⁹ Martha W. Beckwith, *Myths and Ceremonies* (Poughkeepsie, N.Y., 1932), p. 124.

to a traditional phrasing. In his Grandson tale, Grandmother's-knife duplicates the details of rain pattering against the tipi and of the wind heard blowing through the pines, but no other features of his Old Man Coyote tale; and he adds the explicit mention of autumn.

Plenty-hawk's three versions agree in introducing the rustling pines, the wind, the river, and the rain. But there is wide diversity in the ideas evoked by all but the first of these cues. The rain may be described merely as "dripping fast"; but in versions (*b*) and (*c*) it is associated with autumnal precipitation, with the comfort of lying indoors covered with blankets being emphasized. And while (*b*) describes the rain heard against the tipi, (*c*) omits the auditory image but explicitly introduces the return of drenched travelers to camp in the evening. Moreover, (*b*) simply speaks of the fall, which (*c*) further defines in conventional Crow phraseology.

In all versions the river is linked with pitching camp, but (*a*) adds the call of the cicada; (*b*) and (*c*) the picture of shade and of swimming; (*c*) enlarges by explicitly referring to trees, to leaves floating downstream, to the singing of blackbirds over the water.

The wind looms large, either implicitly or explicitly. In two separate "stories" of (*a*) it, respectively, makes the tent ears flap together and causes the rustling of pine needles which latter reappears in (*b*) and (*c*), (*b*) not expressly mentioning the wind in this context. In (*b*) and (*c*) people hear wind blowing, which is of course implied in (*a*).

The differences between the versions by the same informant can thus be grouped under two heads: he may expressly state or suppress implications ("in the fall" *vs.* "in the fall when the leaves have all turned yellow and are falling off"); and he may or may not treat a given situation as a cue for delineating detail (river: shade, scenting of river, trees, floating leaves, singing blackbirds).

Extending comparison to all eleven, we find that the wind figures in every single variant, Hidatsa and Crow, with the solitary exception of the Scratches-face story. A river or brook appears in both Hidatsa and four Crow versions, these, how-

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ever, apportioned between only two narrators. The rustling of leaves (or pine needles) seems to be the only other intertribal feature, being common to Bear's-arm's Hidatsa story and (expressly) five Crow versions (by three informants); interestingly enough, this element appears in all three of Plenty-hawk's otherwise appreciably varying narratives.

Uniformly present in all Crow versions, but absent from the Hidatsa, is the rain element. The most frequent image (five times) associated with it is that of the drops striking against the tipi. Next in frequency are an autumnal scene (five versions, four informants) and the comfort of being warm indoors (five versions, four informants). That comfort may be effectively expressed by contrasting it with the wind or rain or cold outside (Plenty-hawk [b] and [c]; Scratches-face; Gray-bull); and the sense of coziness may be suggested either by the warming of one's soles (Yellow-brow), or by more general references to a fire and warmth indoors (Scratches-face), or to rolling up in a blanket (Plenty-hawk [b] and [c]).

The fascination of these parallels lies in the fact that we are here privileged to catch the folk imagination at work. These Indians are not automatically transmitting a fixed form, even though of course a few phrases have become stereotyped. Instead, there is the tradition of a generic pattern (sleep-inducing conditions) with a few subpatterns (fatigue, shade, specific sounds), which the individual is free to develop as he lists, so that the unique features of the several variants are, in their totality, impressively numerous. What the more elaborate of the versions strive for, evidently, is vividness of description to be achieved by the accumulation of sense impressions, so that we get veritable imagist poems in miniature. The images, however, are not by any means preponderantly visual, but to a very striking degree include auditory and to a somewhat lesser extent kinesthetic ideas.

A Case of Bilingualism

BILINGUALISM, THOUGH OFTEN REFERRED TO, HAS RARELY been described at length. Accordingly, the following notes, though inadequate, may prove of some interest. They are unavoidably autobiographical, for which I apologize.

I was born in Vienna in 1883. My father was a Hungarian from the vicinity of Stuhlweissenburg, southwest of Budapest. In that section of the country German had remained dominant, so that he learnt Magyar as a foreign tongue. My mother was Viennese, and, accordingly, High German was the language of our household. My father's was a generalized South German form, my mother's richly flavored with the racy vernacular locutions which even educated Austrians affect. Typical are such words as *Bissgurn* ("termagant"), *dalket* ("awkward, *gauche*"), *hopatatschet* ("supercilious"). She was capable of expressive original creations, such as *verhallipanzt* ("entangled, confused"), which appears in no *Idiotikon Vindobonense* I have been able to consult. Again, like many educated Austrians, she was somewhat easy-going on certain points of grammar, substituting the dative for the genitive with *während* and *wegen*. On the other hand, her father, a physician, austere criticized such derelictions when

Word, I (December, 1945), 249-259.

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I indulged in them. It was he, too, who urged his daughter to keep up her children's German in America since we were likely enough to learn English there.

When we left Vienna to join my father in New York, where he had preceded us by three years, I was ten and had just passed the entrance-examination for a Gymnasium, my sister being two and a half years younger. We immediately entered public schools and rapidly acquired fluency in English. My mother, obeying her father's injunction, maintained German as the sole medium of communication between parents and children, though my sister and I soon came to speak to each other more frequently in English. The family intimates were all Austrians and Germans, and though our morning newspaper was English, in the evening and on Sundays we regularly bought the *Staatszeitung*. The Sunday edition of that paper had a puzzle-column, over which we pored for hours, winning several prizes in the form of German books. We occasionally went to the two German theatres and in later years visited German societies. We read the classics and the serial modern novels that appeared in our Sunday *Staatszeitung*.

Nevertheless, our German could not possibly develop as it would have in Austria. The range of topics discussed with our parents and their friends did not coincide with that thrust upon us in the classroom and in association with age-mates. It was not as a matter of course, but through later deliberate effort, that I learnt *gleichschenkliges Dreieck*, *Herrentiere*, and *Beschleunigung* as the equivalents, respectively, of "isosceles triangle," "primates," and "acceleration." Similarly, dealings with storekeepers were largely in English. Important, too, was the fact that there were, of course, no compulsory school compositions to be scrutinized by the Argus-eyes of a German pedagogue.

However, the impulse to use our native tongue creatively remained strong. My sister wrote original poems in it and translated at least one lyric by Tennyson; I translated one of Washington Irving's sketches and a passage from Dr. Johnson's *The Vanity of Human Wishes*. Indeed, I made my début in print at fourteen and a half in the *New Yorker Revue* with an article on Edgar Allan Poe, and my first earnings as a writer came from an essay

on Ernst Haeckel in the *Staatszeitung* in 1901. The latter was reprinted without my permission in the anniversary volume dedicated to Haeckel in 1914.¹ I mention the fact because its appearance in Germany indicates that it was grammatically acceptable, though adolescent in style and sentiment. Incidentally, in token of our linguistic conservatism, I note that down to the present we prefer writing German in "Gothic" script, as our mother had always done.

In point of vocabulary my German, as explained, lagged behind my English in various respects, yet it remained ahead of it in the domain of domestic utensils and the like. "Skillet," "rolling-pin," and "saucepan" still click less immediately in my consciousness than *Bratpfanne*, *Nudelwalker*, and *Reindl* (Austrian).

Facility in German composition, of course, implies much more than lexical knowledge; it means, among other things, a control of stereotyped phrases, such as *Beziehungen pflegen*, *Possen reißen*, *Nachruf auf*. . . This is one respect in which the emigrant is handicapped; he knows them, but they are not always at his beck and call; hence, at a pinch, he falls back on a correct enough, but vaguer, colorless expression which a stay-at-home of equal cultivation would spurn.

Grammar presented difficulties of its own. The Austrian vernacular, for example, tabus the imperfect, which it supplants with the perfect. Hence the correct forms of the preterite were matters to be learnt from reading, not through conversational osmosis. Then there are some regional differences as to gender: no Viennese spontaneously says *der Schinken*, but *die Schinke*. Again, perfectly familiar nouns are not likely to be declined often in the ordinary household routine, hence doubts arise concerning weak and strong forms, and *den Hirschen* may usurp the part of *den Hirsch*. Thus, eternal vigilance is the cost of maintaining tolerably good German in a foreign country. We achieved the satisfaction of having our German pronounced much better than that of other children among our acquaintances.

Two vitiating influences had to be specially contended against.

¹ R. H. Lowie, *Was wir Ernst Haeckel verdanken*, ed. H. Schmidt (Leipzig, 1914), II, 404-407.

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On the one hand, an *Auslandsdeutscher* rarely resists the temptation to interlard his speech with alien words. The extreme example is Pennsylvania Dutch. A. R. Horne's *Pennsylvania German Manual for Pronouncing, Speaking and Writing English* (Allentown, Pa., 1896) offers a scene from Hamlet in the vernacular. The ghost reveals himself in these words: "Ich bin deim dawdy si shpook," and the entire passage is of the same order. But the New York German of the less educated immigrants was not much better in the eighteen-nineties. At the Germania Theater I saw a play, "Der Corner Grocer von der Avenue A," whose dialogue was of a piece with the title. But even cultivated people lapse into the use of such convenient words as "ice-box," "car," "elevator." Against this tendency I developed a violent distaste, so that in my early twenties I joined the newly founded New York branch of the *Allgemeiner Deutscher Sprachverein*, which attempted to purge the language of loan-words.

This meant that in extending my English vocabulary I remained restive until I had learnt the proper German equivalent. This was a formidable undertaking, for, especially when traveling in the West and in Canada as an ethnographer, I encountered many things that did not exist in Vienna as I had known it or among New York Germans, e.g., stern-wheelers and narrow-gauge railroads. It gave me a peculiar thrill to learn that they were rendered *Heckraddampfer* and *Schmalspurbahn*, and to the present day a never-failing source of pleasure has been to discover such terms as *Durchschlag* ("carbon copy"), *Kotfögel* ("fender"), and *umschalten* ("to shift gears").

A still more serious, because subtler, peril than the intrusion of English words lies in the spontaneous, unsuspected transfer of English idioms and the misuse of German words because of English models. I once used *nur* instead of *erst* for "only," and on another occasion spoke of having *vermisst* (instead of *verpasst*) a train. Similarly, an Austrian lady wrote about her *Rente* when she meant *Mietzins*, and nothing seems more natural than to *aufrufen* someone on the telephone when usage demands *anrufen*. Lapses of this order always left me with a sense of shame, even when I myself discovered and corrected them.

Still another subtle influence is phonological: the simultaneous

use of similar, but not identical, phonemes in two diverse forms of speech tends to affect pronunciation of the sounds in the medium less frequently employed. On this interesting subject I am able to offer only a single observation. My *r* came to merge with the American *r* after and before consonants. It required deliberate remedial effort to restore the linguo-apical rolling, so that about fifteen years ago the late Professor Prokosch at Yale specially commended my pronunciation of this phoneme.

My German was further modified by contact with many speakers who were not Viennese. Of course, even during my childhood I had occasionally heard North Germans; in fact, one aunt was married to a Mecklenburger. However, such experiences with alien dialects were sporadic. In New York, on the other hand, they were repeated and constant. Our landlord and his wife were Low Germans from Westphalia, the shoemaker was a South German, the tailor a Bohemian. More important, our intimates included two former university students from the Reich, a Berlinese pharmacist, and a Badener of noble descent. Occasional visits to the principal literary society also meant hearing a variety of accents, if not dialects. These influences modified my pronunciation, my vocabulary, even my grammar. I came to lengthen the vowel in *Mond* and to waver between making a terminal *g* (e.g., in *vernünftig*) a stop, as in Austrian, or a fricative, as in stage-German. I picked up unfamiliar words, like *hanebüchen* "coarse." I dropped even in oral communication the Austrian-Bavarian contempt for the genitive in favor of the dative (*dem Vater sein Hut* for *der Hut des Vaters*). I made increasing use of the colloquially lacking preterite.² My German, in short, became synthetic.

I had no opportunity to return to Europe until I was forty-one, when I spent about two and a half months in Germany and Austria. Six years later I visited these countries again for a somewhat shorter period. The principal cities in which I stayed were Vienna, Berlin, Hamburg, Munich, Nuremberg, and Weimar.

The essential facts, then, are that I have preserved a deep devotion to my mother-tongue throughout my life, but after my tenth year, except for a few months, have lived far from a one

² Cf. J. M. Lutz, *Bayerisch* (Munich, 1932), pp. 28-78.

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hundred per cent German-speaking atmosphere. Especially since my removal to California in 1921 and before the advent of refugees from the Nazi régime, oral practice in German was very rare. It now remains to assess the resultant of these factors.

This question is not a simple one. There are many gradations of linguistic proficiency, and this is likely to differ markedly in writing and in speech. If the touchstone is indistinguishability from native usage, we must remember that not all native judges are equally perceptive, and that their conclusions may differ according to whether they listen to the subject when he speaks his vernacular or when he is deliberately transcending it.

Summarizing my own feelings as well as many comments passed on my oral German in the course of years, I should formulate the matter as follows. Naïve, uneducated Germans generally accept me as a German of unspecified origin, for they cannot readily conceive equal fluency on the part of a foreigner. Sophisticated observers detect the Austrian flavor and the synthetic quality of my High German, and tend to identify me as an *Auslandsdeutscher*, as did an educated man with whom I fell into conversation in Hamburg in 1924. In answer to a query, he declared definitely that he had recognized me as one to the manner born, though exposed to alien influences. This accords with my own feelings: I have in the course of my travels met many Swedes, Netherlands, Czechs, and others who spoke fluent and generally correct German, but hardly ever without a sense of my superiority in the use of the language. Naturally even my most stilted High German never suggests a Northern accent to the discriminating; on the other hand, my Viennese has been pronounced authentic by those who ought to know.

Writing, of course, is a different matter. It seems best to discuss this topic later when comparing my proficiency in German and English, respectively.

I was not a complete novice in English when we arrived in New York, since I had for several months taken lessons from an Austrian lady. To be sure, I had not got very far. Her pronunciation was hardly exemplary, for I pronounced *ten* with a long vowel and had no end of trouble with the quality of the *i* in *kill*. Doubtless, too, she followed the German notion that *u*

in *but* corresponds to German *ö*.³ However this be, rapid assimilation of English at once became a primary goal in America. I made it a rule to learn at least ten new words a day. Toward the end of my grammar-school days I conceived an inexplicable admiration for Dr. Samuel Johnson, whose *Rasselas* I read and on whose style I patterned my own with results easily imagined. My teacher in the highest grade repeatedly had to censure my passion for sesquipedalian words. Folklore develops easily among children; I was soon dubbed "the fellow that swallowed the dictionary," and on one occasion I overheard myself described as having gone through the tome as far as H! In imitation of the great lexicographer I even started a rival volume that was to illustrate the use of difficult words by quotations from the English classics.

By the time I graduated from public school my spoken English was superficially not perceptibly different from that of any thirteen-year-old New York boy. Closer inquiry would have established then, as now, the deficiencies already in part alluded to: only a New England wife made me realize the true essence of a "saucepan"; I never encountered the phrase "milling around" until I was on the staff of the American Museum of Natural History; and within the past year I spoke of somebody's being "the split image" (instead of "the spit and image") of someone else. When colleagues credit me with an exceptionally wide vocabulary, I therefore feel bound to qualify the comment. I know many long and unusual words, but I am ignorant of common locutions and not sufficiently conversant with everyday words. In lectures and academic discussions I am fluent enough, but in recounting a simple occurrence of daily life I am likely to grope and fumble for the *mot juste*—say, "running-board" or "dustpan." I constantly marvel at the racy oral English of monoglot New England narrators of moderate education and feel that their achievement is utterly beyond my reach. Incidentally, interlocutors have often chided me for a certain pomposity in speech. In my opinion, this is largely due to my not having the ap-

³ "Deutsche müssen vor allem vor der schauerhaften, gangbaren Schulaussprache mit dem *ö*-Laut gewarnt werden," Otto Jespersen, *Lehrbuch der Phonetik* (Leipzig and Berlin, 1904), p. 156.

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appropriate colloquialism at the tip of my tongue, so that I am driven to seek refuge in a colorless blanket or bookish term.

In apparent conflict with my admiration for the homely authenticity of English speech as spoken by some Englishmen and Americans stands my linguistic authoritarianism. Intellectually I recognize, of course, that "standard" forms are factitious; emotionally I resent deviations. I automatically rank British above American usage and at times wonder at neologisms such as some scholars freely indulge in—say, Kroeber's "formulable," "authenticable." I am shocked by Sapir's defence of accusative "who" and outraged by his repeated use of "nuanced" as though there were a verb "to nuance."⁴ Incidentally, a one-time disciple of his calmly speaks of "sciencing."

Probably because of my bilingualism I do not relish even wholly legitimate latitudinarianism, such as Jespersen prizes as a signal virtue of English. I wish "people" and "committee" were always used with either singular or plural verbs; that a horse were not alternately "it" and "he"; that one could not refer to mankind as "they" (*Oxford Dictionary*) or "it" (common usage) or "he" (Elliot Smith, Rivers).

My pronunciation is not distinctive so far as I can gather, but long residence in the West sporadically affects my *r*'s, which I sometimes suppress and sometimes roll. New words, especially proper names, I occasionally accent erroneously, e.g., "Ha'bak-kuk" for "Habbak'kuk." My wife notes that in comparatively infrequent phrases I tend to stress the noun instead of its substantive modifier, e.g., "home week" instead of "home' week."

The relative attitude of a bilingual toward his two media is not easily determined. Albert Schweitzer, an Alsatian, has offered some valuable hints, which partly but not wholly coincide with my observations.⁵ I agree that it is self-deception if anyone describes himself as having two mother-tongues, i.e., as being able to shift with absolute perfection from one medium to the other in all situations. He suggests a test which no claimant he has known was able to pass: computing with equal facility in

⁴ A. L. Kroeber, *Configurations of Culture Growth* (Berkeley, 1944), pp. 79, 106, 226; Edward Sapir, *Language* (New York, 1921), pp. 103, 166.

⁵ Albert Schweitzer, *Aus meinem Leben und Denken* (Leipzig, 1932), pp. 51 ff.

both languages and designating with equal spontaneity kitchen-utensils or the tools in a carpenter's kit.

In my opinion, however, the phenomenon is more complex than that. Thus, I find no consistent dominance of either English or German. As the preceding pages show, I have actually employed English far more than German throughout the last half century, but I do my sums more spontaneously in German—in fact, until I had to teach school it never occurred to me to do arithmetic otherwise. If I wrote poetry, I think it would be in German.

In reading professional prose I can detect no difference. As to fiction, a test is difficult because one cannot readily find works of equivalent character, hence of similar range of vocabulary. A modern novel, like Heinrich Mann's *Die grosse Sache* (1930), hardly contained unfamiliar words apart from such slang terms as *türmen* ("to scam"), *neppen* ("to overcharge"), *doof* ("stupid"), *pennen* ("to sleep"), *Nutte* ("whore"). But, then, what American reader knows (not merely guesses from the context) the meaning of "harlican," "clacker," "tassets," "chitterling" in Thomas Hardy's *Jude the Obscure*?

I rather think that I have a deeper understanding of German poetry, but here again it is extremely hard to make a fair comparison. The English poet I have read most in recent years is Browning, whereas in German I have reverted to Goethe and sporadically to lyricists of the Theodor Storm period.

In writing or lecturing, my facility is certainly greater in English, for I have literally written thousands of pages in it and lecture six times a week besides conducting a seminar. For a lecture of any consequence in German, e.g., when I addressed the Anthropologische Gesellschaft in Vienna in 1924, I always carefully prepare, so as to preclude the groping for proper words and phrases. However, I found it easy to discuss at least one of the papers read at an international congress in Hamburg in 1930. In ordinary conversation I am likely to be equally halting in both tongues when it is a question of describing a connected series of concrete happenings. I grope for the exact word required and grow embarrassed; or I seize upon an unusual, hence stilted, mode of expression. Amnesia concerning words afflicts me in

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either tongue. A priori I suppose that, on the whole, it is more frequent in German, but I recall one instance when I vainly tried to conjure up English "puddle" and got it only after recalling three German equivalents—*Tümpel*, *Pfütze*, and *Lache*.

To sum up, I am impressed with the difficulty of mastering a single language, let alone two languages, in the fullest sense. Perhaps the situation is different in such countries as Switzerland, but Schweitzer's Alsatian parallel suggests the contrary. Conrad Ferdinand Meyer wrote his masterpieces in German, Turgenev his in Russian, though both had admirable control of French. Rølvaag's novels were first printed in English, but were all composed in Norwegian, though almost all of the author's adult life had been spent in the United States.

There is another aspect of the problem to be considered. A bilingual is the linguistic sample of the sociologist's "marginal man." He suffers in his use of either tongue when judged by the highest standards, but by compensation he has insights not granted in quite so vivid a manner to others. He cannot help constantly comparing modes of expression; and what others recognize as an abstract principle is to him an ever-recurring vital experience—the incommensurability of different languages. This is, of course, obvious when the speakers have evolved different cultural traits, say, the German *Schöffe* or the English *sheriff*. But far more significant is the fact that English does not distinguish between the bleat (*Blöken*) of a sheep and the *Meckern* of a goat; that *gönnen* has to be paraphrased "not to begrudge"; that *Schützling* can be rendered only by the loan-word "protégé." On the other hand, why does German lack a designation for "understatement" or even for so common an action as a human "kick"?

The popular impression that a man alters his personality when speaking another tongue is far from ill-grounded. When I speak German to Germans, I automatically shift my orientation as a social being, I spontaneously adapt myself to the atmosphere characteristic of their status, outlook, prejudices. The very use of the customary formulae of politeness injects a distinct flavor into the conversation, coloring attitudes and behavior. Some of these modes of expression, to be sure, are merely meaningless

formulae, but by no means all. The retention of titles, in European fashion for example, colors mutual relations, as does the free and easy American way of dropping them altogether. In this respect I find myself still strongly under the spell of early influences: whereas my colleagues Kroeber and Olson at once call our students "Jones" or "Cooper," I keep up the "Mr." indefinitely. Similarly in contact with fellow-members of the faculty I am probably never the first to drop the title.

As a marginal man, in short, the bilingual is something of a problem child. It is no surprise to me that "bilingualism has an adverse effect on achievement in intelligence tests."⁶ Language is so intimately interwoven with the whole of social behavior that a bilingual, for better or worse, is bound to differ from the monoglot. His attitude in a novel environment has been forcibly depicted by Rølvaag and, I think, still more poignantly in the following poem by my sister, Miss Risa Lowie, who permits me to publish it:

THE FOREIGNER SPEAKS

You wonder at my diffidence, my wistful smile—
You that never had Whither and Whence to reconcile.

You wonder—whose vernacular rings like true coin,
Whose Present and whose far Past are fields that adjoin—

Why I must ponder simplest things: It must be so—
Twice I must cross the sea on wings for each Yes or No—

Appraising at my far Exchange your casual word,
To see it soar to values strange, or drop absurd—

Returning, amber in my hands, and driftwood blue—
Both gathered on my native strands: but which for you?

⁶ Klineberg, "Mental Testing of Racial and National Groups," in H. S. Jennings *et al.*, *Scientific Aspects of the Race Problem* (London, New York, Toronto, 1941), pp. 260 ff.

Observations on the Literary Style of the Crow Indians

IN ONE OF HIS EARLIER PAPERS PROFESSOR THURNWALD wrote:

Vor allem möchte ich . . . auf die grosse *Verschiedenheit der Begabung* und des *Charakters* unter den Einzelnen hinweisen. Es ist eine sehr verbreitete Ansicht, dass die Charakter-verschiedenheiten unter den Individuen bei den sogenannten Naturvölkern sehr gering sind. Das mag dem flüchtigen Beobachter so erscheinen. Wer aber nicht nur länger mit den Leuten zusammengelebt, sondern sie auch aufmerksam beobachtet hat, dem werden sich die Unterschiede zwischen den Persönlichkeiten wie die unter den Physiognomien aufdrängen.¹

This variability appears very clearly with respect to aesthetic capacity. As Thurnwald incidentally states in a later publication, the Buinese hire "professional" poets to compose songs for them.²

Beiträge zur Gesellungs- und Völkerwissenschaft Festschrift Professor Dr. Richard Thurnwald zu seinem achtzigsten Geburtstag gewidmet (Berlin, 1950), pp. 271-283.

¹ Richard Thurnwald, "Im Bismarckarchipel und auf den Salomoninseln 1906-1909," *Zeitschrift für Ethnologie*, XLII (June, 1910), 146.

² *Idem*, *Profane Literature of Buin, Solomon Islands*, Yale University Publications in Anthropology, No. 8 (1936), p. 6.

In the present essay I wish to offer two versions of the same historical event, possibly dating back seventy years, and to illustrate by them the individual variability of Crow narrative style as well as features that characterize the tribal pattern.

The events described revolve about a highly typical institution, shared by some neighbouring tribes. Although every Crow was expected to be brave, certain men who for some reason had lost all interest in life would deliberately court death. Assuming a distinctive costume and other regalia, such a one would appear in camp, singing a song of his own, saying the exact opposite of what he meant, and expecting others to speak to him in the same fashion. Thus, to make him dance it was necessary to bid him refrain from dancing. By such behavior he gave public notice that he was a "Crazy Dog" (*micgye-wara'axe*) intent on being killed by the enemy during that season. A man of this calibre was highly honoured, but he lost face completely if he tried to annul his promise or evinced cowardice. On the other hand, no ignominy attached to a Crazy Dog who honestly tried to carry out his resolve, but somehow failed of achieving it. Such a one was privileged to renew his pledge the following "season," i.e., during the annual period of tribal reunion following the dispersal into minor groups during the winter. For instance, One Horn's father-in-law was killed during his second assumption of the part.

The motives for becoming a Crazy Dog varied. In the instance just cited the pledger was dissatisfied with the United States Government Agency's distribution of rations. Spotted Rabbit was disconsolate because of the death of his stepfather, Good-Crazy-Dog (*recte* Strikes-the-Enemy-in-His-Brother's-Company) when the Sioux killed some of his relatives. Young Cottontail-Rabbit, the hero of the following narratives, could not get over the handicap of a shattered kneecap, which prevented him from joining in the forays of his age-mates. Naturally there were never many men simultaneously bent on terminating their career. The largest—quite probably apocryphal—number ever mentioned was ten; one informant, One-Horn, remembered as many as five at one time. The usual number seems to have been two, and some years no volunteer was forthcoming. According to Hol-

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man, Knocked-over-by-the-Deer (u'ux-aripu'oc) and Many-Butterflies (Biri'kyac-aho'c) were contemporary with Young-Cotton-tail, but the former got frightened and quit.

My first version was dictated to me in 1907 at Crow Agency by No-Shinbone, to whom I also owe a graphic description of one of his war parties, as well as other information. The second variant was obtained in 1931 from Yellow-Brow, admittedly one of the best storytellers.

VERSION I

[Crow text and accompanying footnotes omitted]

Translation

1. At the Old Agency there goods were being distributed, a Crazy Dog (I then) came to know for the first time. 2. People were seated before the distribution when a young man came riding, holding his blanket close to his stomach and making a rattle of his quirt. 3. Then he entered the camp circle and now I saw the Crazy Dog. 4. Then he sang coming into the circle. "Who is it?" people asked. 5. The young man's knee was swollen, he had been shot, he envied others, he wished to die, that's why he acted thus. 6. Then for a while we never saw him. 7. Then one evening he put on his regalia and came splendidly. The whole camp, we people, were most eager to see him. 8. Then for his rattle, of one of these baking-powder cans he made it, inside he put beads, it rattled mightily. 9. The bridle of his horse had fine chains (?), he put it on, his horse could not be seen because of its trappings. 10. He came, holding his gun by his waist. (For a wrist-band) he put the light-colored end of a tail on his rattle. 11. He himself made his queue, he made little forehead braids for himself, he made himself ear-rings. 12. He put shell on his ear-rings; a necklace he put round his neck, it was exceedingly handsome. 13. His bay horse was bald-faced, it pawed the ground mightily. 14. We saw him, the whole camp liked him. 15. Then he went through the camp, he came singing, he came shaking his rattle. 16. We did not know that he spoke cross-wise.³ 17. He came on. A man said (to him), "Don't dance!" He dismounted at the entrance of a lodge. Then the young man's drummer sang; holding a drum like this one, he sang. 18. He danced. "I'll test myself, I want to die. Whether it will come true or not, I shall know." He shot down at his foot. "It will be well," he said, it is said. 19. The women liked him exceedingly. Every evening he danced. The Crow moved, he sang. When the people had all

³ That is, by contraries.

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camped, he went singing through the camp. The old women cheered him mightily. At night he always sang. 20. When the people wished to hunt, they regarded him as a dog.⁴ He put on his sashes and mounted his horse. All the men went hunting, they wanted to kill buffalo, with arrows they wanted to hunt. 21. The Crazy Dog went hallooing. When these dogs see cattle,⁵ they are wont to run towards them, that is the way he acted. 22. They killed many buffalo, they butchered, they packed their horses. The people pitched their tents, he went singing through the camp. Then the next morning they moved, in a dry gulch with sloping sides we camped. 23. A young man had lost his horse, he turned back, he went to the old campsite, on the site he saw the enemy. He fled, he fell off his mount, the horse ran away. Afoot he reached the camp. "The enemy is in the old site," he said, it is said. 24. Then they (the Crow) charged them. Several young Crow fought, they (the opponents) wanted to kill one another. 25. They drove them (the enemy) back, within a creek-bed they (the enemy) put up defensive works. The Crazy Dog got there, he wished to die. He got to the edge of the defensive works, he shot into them, then they killed him. 26. Then it rained violently. The Crazy Dog was lying in the water. At night he lay there until daylight. 27. The next morning we came there, he was lying in the water. They made wrappings, they brought a horse, they took hold of him, they packed him on (the horse), they led him (the horse). They cried as they were going along, they brought him to the camp. The whole camp cried very much. 28. They planted sticks, there they laid him. They planted a four-pole scaffold and a lodge-pole, to which they tied his sash. Moving without him, we left. That is how he was killed. His drum—like this one—they hung up where they buried. This is the end.

VERSION II

[Crow text and accompanying footnotes omitted]

Translation

1. He had a father, he had a mother. Then his mother died; now he was motherless. It was with his father that he lived. 2. Then his father took a wife; Young Cottontail Rabbit there lived then. 3. This father of his had two children, female ones. 4. Then again he married another woman; the woman, too, had children, two boys. 5. He (Cottontail) went to war; about that I don't know exactly, but he was about eighteen years old. 6. Then Twitching-Eyes was the captain. Way yonder on the other side of the moun-

⁴ That is, they would speak to him as one would in shooing off a dog.

⁵ The Crow word is based on the stem for "buffalo."

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tain they were badgered (by the enemy). His (Cottontail's) knee was pierced with an arrow, the arrowhead did not come out of the kneepan, it was visible. 7. Then they could do nothing, he tried to pull it out, he failed. 8. People came, his leg was swollen, there was nothing like it.⁶ 9. Now they were unable to do anything. "Now, you cannot help me at all, go without me." 10. They made a shelter, they got a great deal of firewood for him, everywhere inside (the shelter) they hung up water-paunches, they killed a buffalo, they cooked a great deal of its flesh. 11. Then this way they went off, there they left him and came away, it is said. 12. When his war party had gone far, they changed their minds, crying all of them turned back. They went on and reached him. 13. "Now then, we have horses, we'll take you along." "No, even if you took me, I'd die. If you don't take me, I'll die; you can do nothing for me. If you walk with me, I should suffer, it would be bad."⁷ Go away, do not turn back." 14. They left, they went on and on and on. They were coming, they reached Pryor, it is said. 15. Of these warriors, a great many were wounded, it is said; those not wounded were few, it is said. Those not wounded reached the path, those who reached Pryor were those not wounded. 16. "Now then, go, get to camp, bring horses." They went in the evening. 17. Then at dawn someone began to sing praise songs, singing he went; this Cottontail's father was the one who sang. 18. The large company was coming, he met this war party. 19. Then the man (said): "What place exactly is he at?" "At Clark's Fork, the trees farthest upstream, there by the rock on the sheltered side among the trees. If you set out now and keep on very steadily, you'll get there at sunset." Then they went off, crying they proceeded. 20. The man (said to himself): "How is he getting on? Is he still living perchance?" he said. Crying he went, he went off, it is said. He got there. They proceeded, they got to where he (Cottontail) was, the sun had just gone down. 21. "Have you still got a body, I wonder?"⁸ "Come, I am not dead yet." He dismounted, he entered. 22. When they noticed it, his (Cottontail's) body was smaller than his leg, his leg was swollen, that was why. 23. There was a man named Breast, he was not a doctor, it is said, but he was experienced, it is said. With him they went, it is said. "Now then, for this we are bringing you, see what you can do about it." "That I'll do." 24. He examined it, he took a bullet mould. "Now, hold him in different parts" (of the body). They did so, it is said. He made the bullet mould pinch the arrowhead, he pulled, he extracted the arrowhead, it is said.

⁶ That is, it was swollen to an extraordinary degree.

⁷ Obscure sentence.

⁸ That is, "Are you still alive?"

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He (Cottontail) fainted, it is said. 25. After a long time he got up, it is said. This man sharpened his knife, he whetted it, he lanced this swelling, all over he riddled it with holes, it is said, he pressed it. The fluid (pus) was yellow, at last it was blood, it is said. 26. He finished. It smarted, he (Cottontail) was out of his senses, towards dawn he was done. He lay sleeping, it is said. 27. The next morning when they observed, the next morning there was nothing the matter with him, it is said. "c. . . ." (sigh of relief), he said, he snorted. He stretched himself. "I almost died." 28. This side of the Old Agency Mountain they were again in a fortified place. Again his knee was wounded, it is said. 29. Then near that Agency, in the area between two joining rivers, there they fought, again his knee was shot, it is said. This knee of his was not straight, it could not be bent, it was bad, it is said. 30. He was foolhardy, yet he was a bashful⁹ young man, it is said, he was kind-hearted.

31. There may have been other deeds, but we don't know about it exactly. 32. After a while he got a wife, she had a child. He divorced his wife, he was wifeless. 33. He was chasing deer, he fell off his horse, his knee hit the ground, his leg again swelled up. They made a travois and dragged it. 34. He suffered from the swelling of his leg. He struck his leg repeatedly. "Ghost-like one, you are making me suffer, (but) by your action you are not going to cause my death," he said, it is said. 35. Then he got well notwithstanding. When young men went on a raid afoot, when they went hunting, when they did anything whatsoever, he could not catch up with them. He was envious about everything; if he exerted himself over something, his leg would swell up again. It palled on him, it is said.

36. When they got to this Agency, they were going to issue goods, it is said. His father had property, it is said, they made him distribute the goods to be issued, it is said. 37. He (Cottontail) took long strips of both red flannel cloth and black flannel. 38. His father said: "What for, seeing that you have no wife? That black cloth was to be owned; you took long strips of it, you are doing wrong." 39. "Why, I want to get married, that is why," he said. The other people spoke: "He wants to marry, it's all right, don't talk against it." 40. Clandestinely he wanted to become a Crazy Dog, that's why he wanted to act thus. He did not tell, he concealed it, it is said. That's how it was.

41. When this issuing was over, some of the camp went in that direction, towards Belt Hill, it is said; half the camp came downstream (east), it is said. They were looking for a fight. 42. He wanted to come with those going east, it is said. To his sisters

⁹ Explained further to mean "of retiring, humble disposition."

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he said: "Come, make moccasins and clothes for me. I am envious over everything. It is a bad business. I am bored. I'll be a Crazy Dog, this summer I want to die." 43. "That is something wrong to do. Even if you do want to die for no purpose, there are many enemies. If you meet some of them in the normal way and are not afraid, they'll kill you. If those who have become Crazy Dogs are not killed, they seem untrustworthy, they seem crazy, it makes them worthless," they said, it is said. 44. "Yes," he did not say, he did not say anything. 45. Then sometime one night when the camp was going to bed, he came out shouting. Then he sang the Crazy Dog song, it is said. 46. These sisters of his fell to crying, it is said. However, they could not help it. He sang, the whole camp had heard him. If he should quit, it would ruin him (i.e., his reputation). "Come, make the clothing for him." 47. He slit a hide, he made it globular, he sewed it, he put dirt in; then, when the dirt was dry, he poured it out and put in beads, half of it he painted yellow, half he made bright red. An eagle tail he put on top of it (the rattle). A fox skin he made into a grip, he sewed little bells all over his rattle. 48. He made a fringe along his temples, he cut his foretop short, for his hair he made a false queue. He wore a very handsome necklace of shell, he had sea-shells for ear-rings; armlets he made for himself. 49. "Half-blue" flannel was hung over his horse, he made a (flannel) sash for his shield, a tiger skin, and forehead discs for his horse. He made long metal chains. He put these on his horse, he made the bridle, then the reins. His horse was a bay with a white face, it was big.

50. Then one evening he fitted himself out handsomely. He cut off his gun, making it short. He made a drum for himself, he made it small, he made it yellow, he slung it over his shoulder. He perforated the butt of his gun and made a support for it; sometimes he put the gun into it (the support), sometimes he carried it next to his waist. 51. It was a fine evening, the ground looked yellow (with spring grass), he was again riding his horse, he came through camp singing, it is said. The people were most eager to see him. 52. He had a high-pitched¹⁰ voice. He was coming, it was some time while he was coming through camp (the Crazy Dogs talk crosswise),¹¹ they said to him: "Come that way, do not dance." "I certainly shall not do so," he said and reined his horse toward them. 53. He dismounted, it is said. He took off his drum, to this man he gave it, it is said. He¹² sang. The words of the song were: "Come, young women, I am coming: when

¹⁰ Thus the interpreter; I should have translated "powerful."

¹¹ Narrator's explanatory parenthesis to clarify the following clause.

¹² The musicians, not Cottontail Rabbit, as shown presently.

the evening is pleasant, I come. Come out!" he said, it is said. He himself did not sing. It was those who made him dance that sang, thereby they made him dance. 54. Then they got to the end of these songs, they sang another one. At the height of their singing¹³ he danced. He jumped around for a while, he took out his gun and shot at the ground, it is said. Then he shouted. He shook his rattle again and again. 55. He remounted. This horse of his pawed the ground all over, with each hoof alternately it pawed the ground. When through, the horse neighed intensely. 56. The Crow Indians were mightily pleased. The young women liked him exceedingly, it is said. While the camp was moving, he would go singing through the camp every evening.

57. At some time there was a very great mutual wife-stealing.¹⁴ Eats-Ears' wife was named Cherry, he (Cottontail) took her. He had been wifeless, now he had a wife. 58. Then some time they were eating, he was eating with his wife, they were eating a tongue. He sliced it and gave it to his wife, it is said. This young woman (exclaimed): "He-hě! (surprise). We are the ones who ought to be doing this surely; he has done it himself," she said, it is said. 59. "Go ahead, take it, when you mourn you will mention it," he said, it is said. This young woman took it and ate it, it is said. She was ashamed, it is said; however, because he had given it to her, she ate it, it is said.

60. Then again some time he said to this young woman: "Why did you marry me? Poor dear, don't you love your forefinger¹⁵ to have married me?" he said, it is said. This young woman sat laughing. "That one I don't love, that's why I married you."

61. Then on a certain evening he came singing through the camp, it is said. When they said, "Come this way, don't dance," he said, "I truly shall not do it" and dismounted, it is said. He (Cottontail) gave him (the musician) his drum, he took the drum and made him dance, it is said. 62. "Come, young women, I have come, when the evening is pleasant I come; come out!" He danced, he got to the very edge of the spectators, smiling he reached the end. It was when the music was at its best, it is said. He went shouting; shaking his rattle, he shouted. 63. He took his gun, he rested it on his foot, then he pulled the trigger, he shot at his foot, it is said. The blood was bubbling; however, he did not mind it, undisturbed he sang through the camp. "It bodes well," he said, it is said; "I was going to die." 64. Unmoccasined,

¹³ "When they sang best."

¹⁴ This took place early in the spring between two rival societies, a member of one being privileged to abduct a former sweetheart who had subsequently married a member of his rival society.

¹⁵ In allusion to her having to chop off a finger joint when mourning his death.

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his foot swelled up, he painted his foot yellow, he did not mind it in the least. In the evening always he would sing through the camp. From this foot he must have been suffering, yet he acted as though not noticing it. He remained unworried, it is said. 65. "It seems we'll never meet the enemy, already we are turning back," he said, it is said. "No, they are crossing ways, we shall meet some of them." Then they turned back at the side of Ballantine, there they camped, it is said.

66. A man lost some horses, he turned back to where they had moved from, it is said. Then he saw the enemy. He came fleeing, it is said. He came, he reached the camp. "The enemy are over there!" 67. Then they (the Crow) made a sudden dash. A few of the Crow reached these enemies. They fought, it is said. Those at home, the main body of Crow also made a dash. They proceeded and got to the enemy, it is said. They fought, it is said. They (the enemy) went down into a hollow, they (Crow) caused them to entrench themselves.

68. For some time the Crazy Dog did not come, it is said. Then he came, he went on, he was close. "There he (the enemy) are staying entrenched," they said, it is said. 69. Cottontail Rabbit spoke: "That is fine, I already thought I would not see any enemies and that we should turn back. There I see some enemies, here is what I seek. When I go, strike your lips in accompaniment." When they proceeded (he said), "We'll dislodge them." 70. Thither he went, he shot into the trench, it is said. At once they hit him in the chest, it is said.

71. They (Crow) took him (the corpse) back, they laid him down away from the river. That night it poured, these enemies stayed around, it is said. Their (the Crows') corpse, Young Cottontail Rabbit, lay in the water that night until daybreak, it is said. 72. The next morning, when they took notice, the enemy—however he did it—had fled and vanished, it is said. This corpse, Young Cottontail Rabbit, they hung over his white-faced horse, thus they brought him back, it is said. 73. They got to the camp with him, they were grieving, it is said. All the Crow, the whole camp, were mourning, it is said. 74. On a four-pole scaffold they laid him; where they laid him they tied a tipi pole to one of the legs, they planted it, to it they tied his sashes; his drum, his rattle they tied to it (the pole). Above they were blowing in the breeze. Then without him they moved.

STYLISTIC COMMENTARY

Certain features of style are inseparable from language, hence both versions are bound to exhibit them. Thus, indirect discourse

is lacking in Crow, hence a person's speech must always be quoted verbatim. In other words, the vivacity conveyed in translation would not be felt by a native audience. A noticeable difference falling into this category is due to a difference in the narrators' circumstances. Crow demands a quotative suffix, -tseruk, when a statement rests on hearsay. Since No-Shinbone speaks as an eye-witness, this particle is not found in his version, whereas Yellow-Brow's bristles with it. Here, however, the matter is not automatically determined: in a long narrative it is not expected that every single sentence be followed by the suffix, hence too frequent use arouses objections, as in the case of another story told me by a third informant, White-Arm. My impression is that Yellow-Brow's style is likewise subject to criticism on this score; we do not encounter a single -tseruk in the opening part of his tale, whereas subsequently it appears over and over again, even in adjoining short predications.

Insofar as the two versions cover the same ground they describe incidents and details very similarly. It is certainly a trivial difference whether the hero's rattle was made of a baking-powder can (No-Shinbone) or in the old style, from a piece of hide molded into globular shape. Both narrators describe the horse as bay and white-faced (No-Shinbone 13; Yellow-Brow 49) and mention his pawing the ground (No-Shinbone 13; Yellow-Brow 55) as well as his metal chains. Young-Cottontail's false hair, necklace, and ear-rings all occur in both versions, indicating the importance attached to such matters by the Crow and their insistence on having them recounted, presumably to give as complete a visual picture as possible. Holman, a middle-aged informant to whom I read the longer variant, ridiculed the failure to mention Cottontail's sashes before the very end. Jim Carpenter was not satisfied with Yellow-Brow's account either. I think he regarded it as too long. From our point of view the essential tragedy unfolded certainly seems somewhat masked by the wealth of detail. Both versions describe the people's approval of the Crazy Dog and the impression made on the women; both mention the circumstances directly preceding his death, the exposure of his corpse, the burial.

Granting that Yellow-Brow, who claimed to have been a

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brother-in-law of Cottontail's (presumably in a classificatory sense), attempts to give the background of the story, whereas No-Shinbone concentrates on the climax, merely referring in lapidary style to the cause of the hero's discontent with life, a characteristic difference appears when the same feature is mentioned. While No-Shinbone simply speaks of the horse's pawing the ground mightily, Yellow-Brow makes him paw with each hoof alternately, then neigh loudly. Judging from other texts secured from these informants, terseness characterizes No-Shinbone, while Yellow-Brow is given to epic breadth.

Yellow-Brow's expansiveness is by no means a wholly negative trait. As in the example just cited, it may add to the clarity of the picture—explicitly stated as a desideratum by the Crow. The dialogue with his wife in itself adds to our conception of a Crazy Dog's state of mind, as does the explanation of his basic surfeit with life (Yellow-Brow 35) and his striking of the knee (Yellow-Brow 34). But this narrator undoubtedly does drag in material quite irrelevant to the central theme, and I find the threefold mishaps to the hero's knee unconvincing. Though I have no express statements from Jim Carpenter on the subject, I wonder whether his dissatisfaction rested in part on certain ethnographical incongruities. I should not expect an adult Crow to address his sisters directly (Yellow-Brow 42); and, more definitely, the query, why Cottontail's last wife married him is pointless: since he kidnapped her in the traditional fashion, she had no choice in the matter. Both narrators assume a certain familiarity with the incidents and, of course, also of Crow culture. Whereas Crow storytellers in general often indulge in excessive meticulousness, they also frequently leave an outsider completely at a loss whom they are talking about. It should be noted that Crow normally expresses the third person simply by the lack of pronoun. It follows that when sentences succeed one another without nominal subjects, the auditor must have previous knowledge of the incidents, for he receives no aid from grammar. For example, a man extracts an arrowhead from Cottontail's body: "He made the bullet mould pinch the arrowhead, he pulled, he extracted the arrowhead, it is said. *He* fainted . . ." The last *he*, of course, refers not to the operator, but to Cotton-

tail. In some instances the proper syntactic significance of a word appears from the context. Thus, grammatically, *isbi'ac* (Yellow-Brow 42), "his sisters," could be the speaker of the following sentence, but since it appears that the speaker wants to become a Crazy Dog and expects his interlocutor(s) to make moccasins for him, "the sisters" are clearly those addressed.

Repetition of the identical word is not regarded as a blemish in Crow narration; and repetition of the same thought in different phraseology is common, as amply illustrated by Yellow-Brow, here as well as in other of his texts. The latter feature is evidently linked with a persistent yearning for balanced structure. To exemplify, we find: "He divorced his wife, he was wifeless" (Yellow-Brow 32). "Those wounded were very many, those not wounded were few" (*ibid.* 15). "His mother died, now he was without mother" (*ibid.* 1).

As for figures of speech, both informants use them sparingly. No-Shinbone compares the actions of the Crazy Dog during a buffalo hunt with a dog's running after cattle (No-Shinbone 21). He also has a hyperbolic phrase when wishing to stress the elaborate horse trappings, which are said to have made the horse invisible (No-Shinbone 9). Still more extreme is Yellow-Brow's statement that the swollen leg was larger than the man's body (Yellow-Brow 22).

A detailed study of all my available Crow texts, many of which remain unpublished, would doubtless cast more light on the literary style of the tribe.

Evolution and Diffusion in the Field of Language

IN GENERAL, NOTHING IS EASIER TO DEMONSTRATE THAN borrowing. Independent evolution is somewhat harder to prove, but in specific instances at least, it can be done. With so many scholars working in ethnology, it is likely that we shall eventually know what was borrowed from whom and by whom. It is also probable that in individual instances independent evolution, or at least an orderly series of reactions to a given set of conditions, will be adequately shown—as has already been done in the continuum: bride service, matrilocality, residence, emphasis upon matrilineal descent, matriarchal clans. Similarly, in the reactions of oppressed peoples towards their oppressors, as evidenced by Christ himself and by many messiahs ever since. Or, on a humble level, by the use of animal dung for firewood by American Plains Indians, camel-breeders of Turkestan, and yak-owners in Tibet; these groups all lived where firewood was scarce, the weather was sometimes cold, and animal dung was plentiful. Various constellations of pressures *do* call forth identical

This paper was found among Professor Lowie's unpublished MSS. Internal evidence suggests that it was written in the last year or two before his death.

reactions in all kinds of peoples. What is true of culture in general is also true for language, since language forms a department of culture, though admittedly a highly specialized and in some respects an autonomous one. The ethnologist is therefore prompted to search in linguistic phenomena for parallels to the processes familiar to him in his wonted fields of inquiry.

There are several relevant questions, of which the most important are: How does evolution proceed in language—assuming that it does? Under what conditions are linguistic features borrowed? Who borrows from whom? Exactly what is borrowed? Why? What happens to the features *after* they are borrowed? What reactions are made as a result of the borrowing? Naturally, not all these questions can be answered for every instance that I shall present, but each of the cases contributes something to a solution of one or more problems.

In linguistics I venture to suggest that some steps *are* predetermined by certain preceding steps. Inasmuch as language grows futile when it ceases to be intelligible, whatever makes for intelligibility is bound to evoke compensatory developments. In his charming little book, *Sound and Symbol in Chinese*, the great Swedish sinologue, Bernhard Karlgren, tells us that in the sixth century Mandarin Chinese had many words that were distinguished in meaning from one another only by terminal stops. "Ka," song; "kap," frog; "kat," cut; "kak," each; were clearly set apart. Then, the last three words, for reasons not explained, lost their terminal surds, creating a number of homophones. When, on top of this, initial sonants became surds, many such pairs of words as "tan," arrive, and "dan," robber, were confounded. Furthermore, vowel sounds earlier differentiated were likewise unified. The total result of all this leveling was that homophones sprang up in appalling numbers; in Mandarin the letter "i" had 69, and the syllable "shi" 59, meanings! Evidently, communication would have been frustrated but for a saving device—the compensatory evolution of tone. This alone did not bring a complete solution but required the addition of what Karlgren calls "special elucidative means." That is, there were still so many homophones that the Chinese had to use synonym

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compounds—such as the pidgeon-English “look-see”—to make their meaning clear.

As Karlgren points out, French offers a parallel in miniature to Chinese homophony in “cou,” neck; “coup,” blow; and “cous,” [I] sew. And von Wartburg adds “ver,” worm; “vert,” green; “vers,” verse; “vers,” toward; “vair,” squirrel fur; “verre,” glass. Undoubtedly, Cinderella’s slipper was made of “vair” (fur) and not “verre” (glass) as in the modern versions. Such convergence militates against the basic aim of speech, i.e., intelligibility, and when it passes a certain threshold, the development must evoke some defense mechanism. If the original lopping off of distinguishing features results from contact with alien groups, then we should have an instance of a diffusion that set in motion a new line of development, although perhaps nothing was actually borrowed directly.

A trite example that illustrates the correlation of descriptively disparate linguistic features is the connection of inflection with the position of words in the sentence. Latin requires no fixed word order because as a rule the nominal endings sufficiently distinguish the subject from the object of the sentence. “Hominem ursus occidit” means the same as “ursus hominem occidit”; neither can be translated “the man kills the bear.” The roles of slayer and victim cannot be reversed, no matter which comes first. Not so, notoriously, in Chinese, French, or English, because there are no endings to tell which is who. Sapir reminds us that the Chinook Indians of the Columbia River were as free as the ancient Romans to use any word order they liked, but not through an identical technique. Instead of allowing the nouns to establish their mutual relations, Chinook imposes a corresponding duty on verbal pronominal prefixes. If a language has no endings for its nouns, something must be done to prevent confusion. English and French solve the problem by using a fixed word order; the Chinook, by a verbal prefix. Given the lack of case endings, one can be certain that *something* will evolve, as an internal necessity. Evolution does, thus, take place in the field of language, although one could hardly predict what form it would take, in any given instance.

Literature, Language, and Aesthetics

Let us now turn to the diffusion of linguistic phenomena. Owing to the limitations of my direct knowledge I shall offer only samples from English, German, and French. Starting from the point of view of the cultural anthropologist, I select from the preceding list of questions the following as of special importance at this point: Who borrows from whom? What is borrowed? Why? and What happens to the loan?

The stereotyped answer to the first question has been that the culturally or politically superior group is the donor, and the culturally or politically inferior group the recipient. However, exceptions occur. The East African Wahima presumably Hamitic in origin, took over the language of the Bantu, whom they subjugated. However, by and large, the principle holds. Politically superior peoples do not necessarily impose their speech upon others, but those who think themselves inferior may voluntarily imitate it. Thus, as Macaulay points out, in the second half of the seventeenth century France achieved "an empire over mankind, such as even the Roman Republic never obtained," combining "almost every species of ascendancy," including linguistic preëminence. French became "the universal language, the language of fashionable society, the language of diplomacy." English authors "affected to use French words, when English words, quite as expressive and melodious, were at hand." As a particularly odious example Macaulay quotes Dryden's lines:

Hither in summer evenings you repair
To taste the fraîcheur of the cooler air.

Now this dominance of French culture was even more pronounced and lasted longer in Germany than in England, because the Germans lacked national unity and national spirit, such as soon asserted itself in England. The petty princes who aped the outer trappings of Versailles inevitably looked upon French literature and language as vastly superior to anything of native growth. Frederick the Great's partiality for French and his withering contempt for German literature are notorious, although he lived at a time when Goethe had begun his career and Lessing was closing his. Educated Germans corresponded with each other in French, and their own language became swamped

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with French words. Since isolated cultural phenomena tend to persist even when out of harmony with new conditions, it is not surprising that the Francomania lingered long after the hegemony of the French state or of French letters. In 1847 Prince Metternich wrote Alexander von Humboldt in French; and it took Bismarck to rule that Prussian ambassadors must report to Berlin in German.

The constant use of two distinct idioms is always a menace to the maintenance of either in complete purity. One reaction to this situation is the familiar mingling of words from both languages, of which the jargon of the immigrant in the United States is a noteworthy example. In my youth the Germania Theater in New York featured a play called, "Der Corner-Grocer von der Avenue A," with dialogue worthy of its title. During the Second World War a scissors grinder, who learned of my Austrian birth, remarked to me, "Wir sind lucky en diesem Country, nicht?" In a Pennsylvania Dutch Manual—issued in 1896 for the purpose of helping Pennsylvania-born people to speak English—the Ghost in Hamlet declares, "Ich bin dei Daddy, sei Schpook," which is neither German nor English. In Goethe's translation of Diderot's *Le Neveu de Rameau* we find two consecutive sentences that are literal renderings of the original French: "Das ist sogar unendlich wahrer, als Ihr selbst nicht empfindet. Ja, so seyd *Ihr andern*." The "nicht" has no place in a German sentence, and "Ihr andern" is obviously a rendition of "vous autres." All these instances indicate reactions on different levels to an intermingling of two radically different languages.

During the period of French ascendancy, as well as earlier, French also borrowed words from German. The French "boulevard" is the German "bollwerk"; the "lansquenet" is a "Landesknecht"; a "chenapan" is a "Schnapphahn." The Swiss mercenaries in the French army also contributed some words, such as "le bivouac" (Biwache), "le cible" (Scheibe), or "le képi" (Käppi). Wartburg tells us that the Swiss soldiers were liable to severe fits of homesickness, hence a merryman, called a *Bruder lustig* was engaged to amuse them, whence French "loutic" (meaning a "wag"), already found in Voltaire.

It would appear, then, that the question of who borrows from whom cannot be answered by a simple statement that the less potent cultural center plays a completely passive role.

In some cases borrowing does not take place at all, when one would expect it to do so. Thus many American Indians have refused to accept English words for new ideas or new objects but have preferred to coin their own terms by appropriate combinations of native stems. The Dakota word for a "horse" is a "mysterious dog," or a "big dog"; the Crow call a cat a "mountain-lion plus dog"; scissors are "two knives"; Sunday is "the day we do not work"; butter is "yellow fat"; a wagon is a "rolling object," and so on. This solution is a relatively hard one, and most peoples have preferred to follow the path of least resistance and take over the word along with the object or idea.

Words may be accepted for any of a number of reasons, some of which have little connection with utility. Sometimes such words become incorporated into the language, and sometimes they are soon lost. The Germans, in their wholesale borrowing of French words, took over *Onkel*, *Tante*, *Cousin*, and *Cousine* for uncle, aunt, male cousin, and female cousin. Naturally, the Germans had always had these relatives, and they had had names for them. These four words have, however, had a different subsequent history. The phonetically inadmissible *Cousin* disappeared altogether and has been replaced by the native *Vetter*. *Cousine* is still heard, but less commonly than *Base*, a native German word. Why the Germans have not reverted to *Oheim* instead of retaining *Onkel*, I do not know, unless it is that the Germanized spelling, with the elimination of the nasal, has made it look like a word of Teutonic origin. But there seem to me to have been the strongest practical reasons for clinging to *Tante*. For, strange as it may seem, German had no unambiguous word for an aunt. *Muhme*, the term now offered by purists, once stood for the mother's sister, while *Base* designated the father's sister. But since the sixteenth century, *Base* has been extended to either type of aunt and subsequently to female cousins, thus precipitating a state of confusion. The at times incredible Eduard Engel in 1918 lists both *Base* and *Muhme* as suitable equivalents of *Cousine*, which does not prevent him from giving *Muhme*

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and *Base* as appropriate substitutes for *Tante*. In other words, no listener could know for sure which relative was meant by either German word. In this situation the French *Tante*, whatever the reason for its original adoption, must have seemed a godsend. Similar matters of convenience have dictated the adoption of many recent loan words into German. English *bond* is used presumably because a business man cannot be expected to say *verzinsbare Schuldverschreibung auf den Inhaber* every time he wishes to express the idea of a bond.

I have, then, after a somewhat sketchy fashion indicated the probable direction of linguistic borrowing and have discussed some of the motives that underlie it. Next comes the question of what happens to the borrowed element, in the course of its absorption into the language. Usually, it is first pronounced with only a moderately accurate imitation, then the approximate sound is spelled according to whatever general principles underlie the native's own language, and it emerges as only a faint image of its former self. Thus, it is said that the strictly Viennese word *hoppertatschet* derives from *de haut en bas*, although by what processes I cannot imagine. There is little doubt that the French *genre* was converted in Austria into *Schan* and no doubt at all that *potschamperl* is *pot de chambre*. However, the Viennese are not the only people who thus convert foreign words into their own system of speech. The French turned "bowling green" into "boulingrin," and the English made "cherry-bang" out of "char-à-bancs."

No less astounding are the transmutations in meaning. French *vasistas*—from German *Was ist das?*—designates a casement, skylight, fan-light, shutter, or blind. *Ressource* in the singular usually denotes a social club in German. The French *plumeau*—feather duster—turns into a feather bed in Austria; *bleu mourant* (pale blue) appears in German as "blümerant," but it means "weak" or "dizzy."

One of the most puzzling things in the conversion of loan words is their change of gender. It is understandable that the English *girl*—used to designate a particular type in German, namely, a chorus girl—should be a neuter, on the analogy of *das Mädchen* or that *Café* is neuter because the German speaker

mentally adds *Haus*. But why, pray, is it *die Garage*, when both *Unterstellraum* and *Schuppen* are masculine? My Duden pusillanimously gives only *das Pokerspiel* thus deciding the gender of *poker* by tacking a good German word onto the loan word, but the less timid *Sprach-Brockhaus* lists *der Poker*; perhaps this gender was selected in consonance with *der Skat* and *der Tarock*, or perhaps someone just drew it out of a hat.

An over-indulgence in linguistic borrowing is usually the forerunner of reaction, suggesting again an internal and independent evolution. I have already quoted a sample of English reaction to too many French words. In this paragraph I will stick to the reaction I know best, since it is similar to that in other countries and among other peoples. By the beginning of the seventeenth century the exaggerated use of French words in Germany had stimulated the beginnings of a counter-movement. In Weimar in 1617 a society was founded by Prince Ludwig von Anhalt-Köthen and three Saxon dukes for the express purpose of "purifying" the mother tongue. So far as I can find out, it led to no concrete improvements and had run its course by 1680. This society had successors, but none of them seem to have left much impression upon the language, although they doubtless served to keep alive the spirit of revolt. However, Joachim Heinrich Campe (1746-1818), a pedagogue and author of juvenile books, who in 1801 published a *Wörterbuch der Erklärung und Verdeutschung der Unserer Sprache aufgedrungenen fremden Ausdrücke*, followed by a *Wörterbuch der Deutschen Sprache* (1807-1811) is credited with such felicitous and current coinages as *Zerrbild* for *caricature* and *Stelldichein* for *rendez-vous*. With the rise of nationalism after the Franco-Prussian War the urge toward purification grew and led to the founding of the *Allgemeine Deutsche Sprachverein* in 1885, at the suggestion of the art historian Hermann Riegel. Among its aims it proclaimed the purging of unnecessary alien linguistic ingredients and the consequent strengthening of German unity. Besides a series of Germanizing glossaries for various phrases in social life, commerce, or medicine, it published a journal and actually succeeded in influencing the vocabulary of the press and of German officialdom.

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What struck me, as a one-time member of the organization, during recent trips up, down, through, and across Germany was the apparently complete disappearance of this relatively harmless form of nationalism. It is true that a few once-common French words have disappeared; one no longer hears *Adieu* or *Trottoir*, for instance. But from Hamburg to Munich and from Bremen to Beyreuth there is no *Haarschneider* or *Haarkünstler* or even *Barbier*, only *Friseurs* or *Coiffeurs*. The *Speisekarte* or *Speisezettel* has receded before the *Menu*, which bristles with such items as "schaschlik à la Russe," "Compôte," "Pommes Dauphine," "Rumpsteak Grandemère," and the like. When one wanted oatmeal, the waiters were more likely to understand if one asked for *porridge*; a request for *Haferflocken* merited only a blank stare. In the newspapers and in everyday speech one constantly heard such English or American words as: best-seller, slogan (somewhat camouflaged in speech behind the Teutonic pronunciation of "Schlo-gan"), comeback, knockout, bluff, screen star. From all of this I conclude that puristic fervor is a thing of the past.

If one wishes to observe borrowing in a country where there seems to be no barrier against it, one should go to Switzerland. The German Swiss are serenely indifferent to the purity of the standard German that most of them can speak when they want to. The frequently heard "Merci vielmals"—with both words mispronounced—may be taken as a symbol of their attitude. Just as the Austrian calmly takes the technical botanical Latin "ribes" (for "currant"), tacks a diminutive suffix to it, and comes up with *Ribisel* for "grapes," so the Aleman—witness Jeremias Gotthelf—seizes upon *trousseau* and makes *Trossel* out of it.

Yet the Swiss is an intransigent purist, but only as regards his *Schwyzerdütsch*. I should perhaps explain that *Schwyzerdütsch* is not a dialect of German, but a distinct Alamannic language, split into many dialects, of which Bernese is the most aberrant. When one wishes to speak the number "two" in Basel, one says *zwie*; in Zurich, *zwo*; in Bern, *zwee*, and in Chur, *zvu*, but never *zwei*. All German-Swiss learn standard German in school, and their newspapers are printed in it; but, irrespective of their educational level, they abstain completely from its use among themselves, except in the university lecture hall. What the Swiss

purists fear—to judge from frequent indignant letters to editors—is an intrusion of standard German into their dialects, which they wish to keep undefiled.

Language is a realm by itself for the linguist, who contemplates its phenomena rationally, but for the mere speaker it is charged with emotional connotations of frequently terrific power. Minor deviations in pronunciation from whatever is regarded as standard precipitate ridicule, contempt, suspicion, and antipathy. Linguistic differences turn into cultural phenomena, into sub-cultural differentiae, into class badges. The Englishman who drops his *h*'s is an inferior social being, and Mr. Acheson is a suspicious character because he speaks with a "Harvard" accent. Thus, language becomes, in its effect upon those who use and hear it, a proper subject for the ethnologist's attention.

Moreover, one can see in the field of language the same phenomena that one finds in other areas of culture. Sometimes changes are instigated by previous developments within a language—as in the case of the Chinese homophones—sometimes they occur as a result of outside pressure—as when the Plains Indians invent their own words rather than borrow those of the white man—and sometimes they are touched off by a supposed insult to national pride through the adoption of too many foreign words—as in the German example above given. The same kind of development has occurred more recently in Turkey. Often language becomes a symbol of nationalism and passes through an epoch of intense emotional reaction. Excesses in one direction lead to counter-blasts; and through it all the man-in-the-street follows his naïve human urge towards minimal effort and usually manages to offset the intentions of the intellectuals. Even when borrowing does not arouse resistance, it is not a routine, simple act. The borrowed element is gradually absorbed into its new linguistic background, altered in pronunciation and spelling, and perhaps in meaning also. Therefore, change begets more change, and one sees in language the same interplay of diffusion and evolution that occurs in any other area of human development.

PART III *Relation of Ethnology to Other Disciplines*

The seven papers that constitute this section can only adumbrate Lowie's awareness of the contribution ethnology might make to other fields. His interest in biology, human geography, and history are only glancingly revealed. His interest in psychology was real and persistent, but his conception of that field was essentially atomistic and rationalistic. Lowie saw people as "congeries of traits," and he distrusted the worth of the various schools of depth psychology that informed so much of the research in "personality and culture." Economics he understood largely in terms of property or ecology. In political science his interests focused primarily on social controls. "Property Rights and Coercive Powers of Plains Indian Military Societies" (No. 20) and "Some Aspects of Political Organization among the American Aborigines" (No. 21) illustrate this point, particularly when read in conjunction with his book *The Origin of the State*, published in 1927. These papers, like his much esteemed *Plains Indian Age-Societies: Historical and Comparative Summaries* (AMNH, Anthropological Papers, XI, Part 13, 1916), illustrate Lowie's nice appreciation for specific linkages between institutions and between fields of inquiry. They illustrate also his consummate skill as an ethnologist, in letting his data lead him rather than forcing them into preconceived categories.

Psychology and Sociology

WHAT ARE THE RELATIONS OF PSYCHOLOGY AND SOCIOLOGY?

It is clear that the sociology of both primitive and higher civilizations yields new data for psychological interpretation. But can psychology as the older science, dealing with more fundamental phenomena, throw *any* light on the problems that confront the sociologist and ethnologist?

The question, even in this drastic form, is hardly absurd at the present stage of sociological and anthropological thinking. On the one hand, we find Graebner, the leader of the German historical school, resolutely turning his back on anything that savors of psychological interpretation. The sum and substance of ethnology, he tells us in his *Methode der Ethnologie*, is to determine the actual development of cultures; and this he forthwith outlines as the result of contact between different peoples, leading to intermixture and superposition of cultural traits. From this point of view any similarities observed in different regions must be traced to a single point of origin, for there is no criterion, no certain proof, of independent development, while cultural borrowing is not only in some cases an established historical fact, but may be considered demonstrated when a

American Journal of Sociology, XXI (September, 1915), 217-229.

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resemblance of form between the particular features compared is accompanied by a corresponding similarity of associated traits. It matters not to Graebner whether a division of society into exogamous moieties may mean one thing in Australia and quite a different thing among the Iroquois or the Tlingit. He is interested in classifying cultural results, and one moiety appears no different from another. If a bungling schoolboy by a double blunder attained the same sum as a calculating-machine, Graebner would doubtless accuse either boy or machine of copying.

Very different is the position assumed by such writers as Lévy-Bruhl, Rivers, and Wissler. Each of them would insist that it is not a purely objective cultural epiphenomenon that we are dealing with in culture, but that it is precisely the subjective aspect of the problem that tempts and repays study. The opposition of the writers mentioned to current psychologizing rests on very different motives and seems to be associated with certain notions as to the hierarchy of the sciences. Precisely as many biologists now hold that vital phenomena cannot be reduced to physics and chemistry but require a distinctively biological explanation, so eminent sociologists and ethnologists now tend to believe that sociological data are *sui generis* and defy interpretation by individual psychology. The *collective* ideas encountered by the sociologist, thinks Lévy-Bruhl, are generically different from the ideas evolved by the individual mind and obey laws other than those derived from an analysis of individual psychology. Similarly, Wissler has suggested that psychological and cultural processes belong to different levels or cycles and should be interpreted independently of each other. Rivers, to be sure, does not exclude the possibility that for an ultimate explanation of cultural data recourse may be had to psychology. Nevertheless he, too, insists that in the treatment of immediate problems we must attempt "the correlation of social phenomena with other social phenomena, and the reference of the facts of social life to social antecedents."

Anyone who has delved into the semi-popular ethnological literature of, say, the last two decades will hardly fail to sympathize in very large measure with the views just cited. The cheap plausibility about many current attempts to bring primitive

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or modern social thought nearer to us has been admirably exposed by Lévy-Bruhl. Yet the trouble with many of these interpretations is not that they are psychological but rather that they are folk-psychological: they rest, not on the established results of scientific psychology or at least on points of view that have proved fruitful within that science; but rather on the sort of offhand guesswork with which in everyday life we attempt to fathom the motives and thoughts of our neighbors. And even where the ethnologist does not indulge in this form of popular psychologizing he is likely to offer as a psychological explanation what cannot by the most strenuous exertion of the will be twisted into the semblance of one. An example is furnished by Professor Kroeber's "psychological explanation" of kinship terminologies. Professor Kroeber has it that relatives are not classified according to social but according to psycholinguistic categories, which he lists accordingly. His enumeration is one of the most notable feats in the history of the subject, but in what way has it anything to do with the science of psychology? What psychological processes cause many peoples to classify collateral and lineal relatives together, or to use a distinctive set of terms for a male and for a female speaking? These are linguistic phenomena that may call for a psychological interpretation; but merely to say that psychological factors have been at work is not producing the factors (such as we know from our textbooks on psychology), is not, then, a psychological explanation at all.

Yet, when all is said and done, the spirit of skepticism that has invaded sociological and ethnological circles may be carried too far. I venture to believe that some facts may not only become more intelligible when viewed from the angle of individual psychology, but it may be advisable not to defer this mode of looking at them until an indefinitely remote future. Even where individual psychology has not yet advanced far enough to give a solution of the problem, the new data may well prove a goad for further development of that branch of the science. And again an ethnologist conversant with psychology may give a more accurate description of his observations than his less sophisticated colleague.

All this becomes clearer by concrete illustration. I will begin by offering some remarks on a subject that figures largely in discussions of religion—dreams and related phenomena. Every sociologist knows of Tylor's attempt to account for the savage belief in a hereafter through visits from or to the dead as experienced in dreams or visions. This theory can of course be assailed on Lévy-Bruhl's principles; it may be said that an individual will interpret his dream only in a way more or less predetermined by the mode of thought current in the society about him. But the point at issue may also be approached by the avenue of individual psychology. Some psychologists, such as Radestock and Wundt, have not hesitated to accept Tylor's theory at its face value. They find it perfectly natural that the thoughts of surviving relatives should continue to busy themselves with the recently deceased, and that accordingly their dream life should be haunted by the figures of those who have just departed. Nevertheless this argument is no more than a piece of plausible folk-psychologizing. Yves Delage, on the basis of personal observations, arrived at the conclusion that ideas which preoccupy the mind in waking do *not* appear in dreams and that one does not dream of important events of life except when the period of preoccupation has ceased. More particularly he found that one does *not* tend to dream of a recently deceased relative.¹ With qualifications that seem immaterial in this context Delage's views are corroborated by Professor Mary Whiton Calkins' "Statistics of Dreams."² She, too, finds a strong tendency for unimportant events of waking life to crop up in dreams, while events of real significance occur with amazing infrequency; and her independent examination of dream records confirmed Delage's special point as regards the apparitions of the recently deceased.

The particular facts of this case are of course unessential. I have not followed recent dream-study sufficiently to be able to vouch for the correctness of the views cited. But one thing is clear. The sociologist who is acquainted with Delage's and Professor Calkins' observations will avoid the pitfall of a "psychological"

¹ Yves Delage, "Essai sur la théorie du rêve," *Revue Scientifique*, XLVIII (1891), 40 f.

² *American Journal of Psychology*, V (1893), 311-343.

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interpretation that might otherwise seem axiomatic; the knowledge of what at least some inquirers have advanced against that interpretation will serve as a prophylactic against accepting plausible guesswork for scientific truth.

So far, to be sure, scientific psychology carries us no farther than Lévy-Bruhl's collective ideas. We have argued the merits of a particular psychological explanation from its own point of view and found it wanting; Lévy-Bruhl's principle would preclude error by simply shutting out *any* explanation of this type. In order to vindicate the claims of scientific psychology in sociology we must therefore prove that it has something more than a purely corrective value. This additional function consists partly in the more accurate determination of facts. Ethnological and sociological literature fairly reeks with such phrases as "crowd psychology," "hypnotism," "suggestion," "influence of dream life," yet rarely are these terms more than exceedingly loose and misleading catchwords. To stick to the last-mentioned topic, in ordinary savage parlance such phenomena as "dreams" and "visions" are often thrown together under a single term. Here it is the duty of the field worker to discriminate lest his record become worthless. The difference between the religious life of two tribes may center precisely in the fact that in one of them supernatural revelations are sought through artificially induced visions while in the other they come through the natural medium of dreams. And in either case by no means all the experiences are of the same significance. We know that among the central Algonkian tribes revelations through visions may be declined under the influence of the preconceived notion that a particular kind of revelation must be secured. Here, clearly enough, a collective idea overrides the individual psychological experience, but in order fully to appreciate the significance of this very fact we must know definitely what the individual experience has been, and any analogous instances from psychology and psychiatry are of value. If, on the other hand, communication with the spirit world takes place through dreams, the question arises *which* dreams become significant, and here an intensive psychological analysis may become necessary. An ethnologist who knows what is going on in psychology may ask whether the dreams that are

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culturally important conform to certain types suggested by current psychological discussion, say, whether they present the Freudian character of a repressed wish fulfilled. It may, of course, turn out that the dreams in question are wholly predetermined by social thought; but this should be the *result* of the investigation, not a foregone conclusion. Thus, scientific psychology may assist in greater precision of statement as to recorded facts and prevent the lumping together of disparate phenomena; and it may further suggest lines of inquiry closed to those not conversant with what psychologists are doing.

A still more important service may be rendered by psychology in connection with the ever-vexing problems of the unity or diversity of origin of similar cultural traits. Discussion of this point has always loomed large in the annals of anthropology; in recent times it has become the storm center in the whirlwind movements of the Graebnerian school. For, as already stated, Graebner denies that there are any objective criteria of independent development. To say, for example, that the same mythological ideas may develop independently in different parts of the world seems to him worthless, subjective twaddle. The thing is conceivable, he admits, but this does not prove that it has really taken place. Now, as I have pointed out elsewhere,³ this is true but applies in equal measure to the supposedly objective proof for historical connection. Here, too, what can be demonstrated is simply the fact that two features are similar; that such similarity means unity of origin is pure inference, not a whit less subjective than the alternative hypothesis of diversity. But in weighing the evidence pro and con we cannot but attach great significance to whatever results scientific psychology may have ascertained as to general traits of the human mind.

Take, for example, the hideous ogres that infest the mythological world of widely separated races. Shall we accept the conclusion that they took shape in a single locality and thence spread over the entire globe? If so, how did such unrealistic figments of the imagination arise? And—even if we choose to ignore the psychology of origins—why should such fantastic

³ R. H. Lowie, "On the Principle of Convergence in Ethnology," *Journal of American Folk-Lore*, XXV (1912), 24-42. This paper is reprinted as No. 23 of the present work.

imagery be uncritically adopted the world over? These questions are at least answered intelligibly, if not correctly, when we learn that the unrealistic figures of mythology do occur rather frequently in dream life. For even if their *origin* should not be traced to this source, we can at least understand why a type of imagery familiar from dreams should be accepted as part and parcel of a conceivable mythic world. This seems, indeed, to be the verdict of psychology. Wundt distinguishes a type of dreams peopled with grotesquely distorted shapes: there are faces with enormous probosces, projecting tongues, and gnashing teeth, while the head may rest dwarf-fashion on a stunted body. On apparitions of this type, Wundt believes, have been patterned the Gorgons and satyrs and pygmies of mythology.⁴ Other students support the general psychological fact. Maury often saw in dreams a sort of green-winged bat with a red head and a grimace on its face. Mourly Vold reduces all these phenomena to a psychologico-psychological basis: in sleep tactile and motor sensations give rise to visual hallucinations, embracing those of the type now under consideration.⁵

However cautious we may be about accepting Wundt's interpretation as a final one, it is clear that the case for the theory of independent development becomes very much stronger when we find that the strange ogre figuring in myths can and does recur in individual dream life over and over again and may be referred to rather definite physiological conditions. As against Graebner we have thus scored a point. But the indefatigable disciple of Lévy-Bruhl who is dogging our footsteps may object that when an individual dreams of, say, a Gorgon, it is because the Gorgon is a "collective idea" common to his social group, an idea with which his mind has been saturated since infancy and which thus naturally appears in his dreams. In other words, the phenomenon is essentially not psychological but sociological: as a modern philosopher inverted the commonplaces of materialism by inquiring why the mind has a body, so Lévy-Bruhl's follower nowhere sees products of individual minds becoming socialized but only social ideas shaping individual thought.

Here a twofold answer is possible. In the first place, to abandon

⁴ W. M. Wundt, *Völkerpsychologie* (Leipzig, 1900-1909), II, Part 2, pp. 114-118.

⁵ Nicolas Vaschide, *Le Sommeil et les rêves* (Paris, 1911), pp. 197-225.

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a psychological explanation for the Gorgon is to abandon *all* explanation; the Gorgon enthroned as a collective idea may be inexpugnable but it is also incomprehensible and barren. Collective ideas are blind alleys; to make headway we require the admittedly fragile aircraft of individual psychology. Secondly, Lévy-Bruhl's theory involves as an essential part the doctrine of the diversity of human psychology. Why, then, we may reasonably ask, have diverse social groups produced similar mythological concepts? To this no answer is forthcoming from the opposite side.

Let us turn now to another field of inquiry. For a long time ethnologists have been struck by the odd mode of associating ideas found among primitive peoples. In some cases, to be sure, the peculiarity may be due merely to our ignorance of an intermediate link that has dropped out. When I am told by a Hidatsa Indian that the maize he plants and the wild geese he shoots are one and the same thing, I am puzzled; but when I learn that both maize and wild geese are attributes of the same mythic character, a logical and possible (though not necessarily the historically correct) bond is supplied. So an educated Hindu might wonder at the emotional suggestions of the cross, but they would at once become intelligible from scriptural history. However, in most instances the search for the missing link seems hopeless; and, what is more important, the very principle of such a search seems subject to doubt. For it assumes that there is a rational bond, while the trend of modern research is certainly to emphasize not logical but "pre-logical" associations and to view the rationalistic as a secondary, superimposed feature.

Before going farther it will be well to cite some examples of the types of association I have in mind. Among the Crows I have been told that everything in the universe exists in fours. As a matter of fact, the predominance of Four as the mystic number is very striking, not only in this tribe, but throughout an immense region of North America. Processions must take four stops; songs must be sung in sets of four; mythic heroes accomplish miraculous deeds at the fourth trial, and so on. In Oregon, the place of Four is taken by Five, while in the Old

World both are overshadowed by Three and Seven. M. Lévy-Bruhl has well expressed the essential fact in all these cases by describing the mystic numbers as categories into which reality is fitted: "Au lieu que le nombre dépende de la pluralité réelle des objets perçus ou imaginés, ce sont au contraire les objets dont la pluralité se définit en recevant sa forme d'un nombre mystique fixé d'avance."⁶

But it is not merely numbers that are associated with apparently fanciful ideas. To a Crow a diamond represents a navel cord; a rectangle, quadrilateral, right angle, and certain combinations of figures suggest to the Arapaho the notion of life and prosperity; and among the western Dakotas a form of lozenge symbolizes the whirlwind.

Finally (for our present purpose), there are strange associations with color. In addition to color associations that are self-explanatory, such as the connection between red and blood, white and snow, green and grass, there are others of a puzzling character. In several Plains tribes black symbolizes victory and joy; the Cherokees associate white with the south, red with the east, black with the west, and blue with the north; the Dakotas symbolize both the north and the south by blue, etc.

How are we to account for such associations? The interpretations usually given are manifestly unsatisfactory. It will not do, for example, to say that geometrical designs are derived from realistic representations of objects in nature through a process of degeneration, the name of the original model having been retained for the conventionalized, geometrical form. First of all, this does not account for symbolism of an abstract character. Secondly, it has been found that often the same pattern symbolizes one thing in one tribe and another in a neighboring tribe, or even different things within the same tribe. As for numbers, it has been suggested that the mystic qualities of Four are due to the existence of four cardinal directions and winds, the idea of which is again associated with four sacred animals, colors, and what not.

Lévy-Bruhl has given an admirable critique of this and other

⁶ Lucien Lévy-Bruhl, *Les Fonctions mentales dans les sociétés inférieures* (Paris, 1922), pp. 256 f.

so-called psychological theories of the same nature. In the primitive mind, he argues, there is no conception of north as a spatial division, with west at the left and east at the right, to which there are subsequently added the ideas of cold winds, snow, the bear, blue, etc. Rather are all these ideas bound up in a single complex collective idea, with the mystic elements masking those which *we* call real, and within this complex is comprised the element Four. When the mystical "participations" are no longer felt, there are precipitated the associations that everywhere persist in some measure. *Now* they are associations because the inner bond that integrated them has disappeared; but originally they were of quite a different character.⁷ Again, the mystic properties of Seven among the Malays have been derived from the fact that the Malays believe that man has seven souls. Arguing in a way that must be absolutely convincing to every unbiased ethnological thinker, Lévy-Bruhl inverts this supposed explanation. Seven does not play the part of the mystic number because the Malay believes in seven souls, but the Malay believes in seven souls because the preëxisting numerical category pre-determines his speculations as to the number of souls.⁸

Nevertheless this point of view cannot be a final one. It may be that the Malay conception of Seven has been an established category for untold aeons, and that the complex collective idea of Four is of corresponding antiquity in North America. Nevertheless, somehow and somewhere these complex "collective ideas" must have taken shape in an individual mind; to "explain" them psychologically, i.e., to class them with related phenomena of individual psychology, seems to be indispensable for a proper understanding of the facts.

In his *Inquiries into Human Faculty and Its Development*, Francis Galton has collected data at least generically related, I believe, to those under discussion. He found that imaginative persons almost invariably think of numerals in some form of visual imagery.

If the idea of *six* occurs to them, the word "six" does not sound in their mental ear, but the figure 6 in a written or printed form

⁷ *Ibid.*, p. 242.

⁸ *Ibid.*, p. 250.

rises before their mental eye. . . . Those who are able to visualize a numeral with a distinctness comparable to reality, and to behold it as if it were before their eyes, and not in some sort of dream-land, will define the direction in which it seems to lie, and the distance at which it appears to be. If they were looking at a ship on the horizon at the moment that the figure 6 happened to present itself to their minds, they could say whether the image lay to the left or right of the ship, and whether it was above or below the line of the horizon; they could always point to a definite spot in space, and say with more or less precision that that was the direction in which the image of the figure they were thinking of first appeared.

To a person of this type, series of numbers arrange themselves "in a definite pattern that always occupies an identical position in his field of view with respect to the direction in which he is looking." These patterns or "forms" vary individually, but are stated in all cases to date as long back as the memory extends, to come into view independently of the will, and to be nearly constant for a given individual. Moreover, there is the strongest evidence that the peculiarity is hereditary "after allowing and over-allowing for all conceivable influences of education and family tradition."

Galton discovered not only an association between series of numbers and definite patterns, but an additional association, in some cases, between series of numbers and colors. And what is perhaps of still greater immediate interest for the present purpose, he found that numbers are often personified and invested with a definite character. Three was described by different informants, respectively, as a treacherous sneak, a good old friend, delightful and amusing, etc. Galton himself "had absurdly enough fancied that *of course* the even numbers would be taken to be of the male sex, and was surprised to find that they were not." The association of color with sounds had been known prior to Galton. Galton notes cases of the association of definite colors with certain letters and with certain days of the week. One of his correspondents not only associated letters with colors, but conversely collected "scraps of various patterns of wall paper, and sent them together with the word that the colour of the several patterns suggested to him." A blue bottle-shaped design on a

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like background suggested "sweet," yellow leaves on a yellow-red background striated with black vertical lines meant "range."

The psychological phenomena presented by Galton seem to me, I repeat, connected with the cultural phenomena under discussion. The association between a blue bottle design and sweetness does not seem to differ generically from the Dakota's association of a lozenge with the whirlwind. If an English-woman thinks of Tuesday in association with a gray sky color, while Friday suggests a dull yellow smudge, why should not the Indian associate the north with blue and the south with white? And if numbers are endowed with individual personalities by Europeans,⁹ what is marvelous in the fact that primitive tribes attach a preferential estimate to one (or, it may be, more than one) particular number? To be sure, the nature of *all* the associations, individual as well as sociological, is obscure, i.e., irreducible to a *logical* basis. But we have at least classified the sociological phenomena with those phenomena of individual psychology that are akin to them. For that very trait emphasized by Lévy-Bruhl as characteristic of the sociological ideas, to wit, their initially complex character, is in the highest degree characteristic of the Galtonian phenomena. The letter A is not first conceived independently by a Galtonian subject and *afterward* associated with a color. To the subject "A-brown" is an ultimate datum, "*une représentation complexe*," which can indeed be analyzed by the psychologist, but the analysis of which cannot, without committing the psychologist's fallacy, be projected into the subject's psychological experience.

To avoid misunderstanding, a word as to the relation of the psychological and sociological elements in a concrete case may be desirable, even at the risk of repetition. When a Crow Indian originating a new ceremony prescribes *four* sacred songs, his psychological condition with reference to Four may be quite different from that of an individual to whom Four appears as the incarnation of everything good and beautiful. He may be individually quite indifferent to the number Four; and even if he were not, his attitude toward it would be inextricably bound up with his attitude of unconsciously bowing to the traditional

⁹ This trait is shared by me.

category. In other words, his psychic state is characterized, in all probability, not by a spontaneous reaction to Four, but by a spontaneous reaction to the tribal lore. Substitute Three for Four as the tribal mystic number, and his psychic reaction would not vary a jot. We may go farther. Owing to the wide distribution of Four as the mystic number, it would be rash to assume that its use as such originated with the Crow Indians. Hence we are probably dealing, not only with the sociological problems of the predetermination of individual reactions by the social group, but also with the psychological problem of a social group borrowing a cultural phenomenon from another group which, for the sake of simplicity, we will assume to be the originator. Now, within this hypothetical group, I repeat, the endowment of Four with certain attributes must somehow have taken shape in an individual mind, and the acceptance of that individual evaluation of Four—its promotion from a psychological to a cultural position—is an example of the influence of the individual on the group. That acceptance becomes the more readily intelligible when we recollect the highly hereditary character of the Galtonian phenomena and the fact that primitive communities are very largely constituted of blood relatives.

My general conclusion as to the relation of psychology to sociology may therefore be summarized as follows. There can be no doubt that the psychological interpretation of cultural data is fraught with serious difficulties. We have not only to disengage the psychological fact from complicating conditions of a historical order, but we must also reckon with the additional obstacle that the individual psychic phenomenon as it confronts us has already been in some way molded by sociological factors. We may, of course, cynically eschew any and every explanation of the subjective aspect of culture. If we are not content to mortify the spirit to this extent, we have no choice save between popular and scientific psychology. Scientific psychology will not solve all our sociological problems, nor many at the present time, but while not omnipotent neither is it powerless. It will not only act as a corrective in speculative interpretation, but will lend greater rigor to our formulation of fact and open new prospects of inquiry and explanation.

Oral Tradition and History

A LITTLE OVER A YEAR AGO I PROTESTED AGAINST THE acceptance of oral traditions as historical records.¹ I held then, as I do now, that those who attach an historical value to oral traditions are in the position of the circle-squarers and inventors of perpetual-motion machines, who are still found besieging the portals of learned institutions. The discussion precipitated by my remarks in the journal mentioned,² and still more a great many private debates with fellow-students, have not shaken my confidence in the soundness of the views previously voiced; but they have shown conclusively that I had misconceived the psychology of the situation. Instead of being a high-priest hurling anathemas against the unregenerate heathen, I found myself a prophet preaching in the wilderness, a dangerous heretic, only secretly aided and abetted by such fellow-iconoclasts as Drs. P. E. Goddard and B. Laufer. I cannot regard it as a healthy condition of affairs in science when the adherents of antagonistic

Address of the retiring President, delivered at the Annual Meeting of the American Folk-Lore Society in New York, December 27, 1916. Printed in *Journal of American Folk-Lore*, XXX (April-June, 1917), 161-167.

¹ "Oral Tradition and History," *American Anthropologist*, XVII (1915), 596-599.

² *Ibid.*, pp. 599-600, 763-764.

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views see no virtue whatsoever in each other's position. Perchance there is some hidden source of misunderstanding that only need be revealed to make co-existence, if not amity, in the same logical universe, possible. I therefore avail myself of the present opportunity to present without primarily polemical intent the logical issues as they present themselves from my angle of vision.

In the first place, it may not be unnecessary to state that in denying to oral traditions of primitive tribes their face value, we are not denying to them *all* value whatsoever. On the contrary, it is clear that even the wildest and manifestly impossible tales may be of the utmost importance as revelations of the cultural status of the people who cherish them, whether as annals of incidents that once occurred or as purely literary products of the imagination. In addition to this willingly granted psychological significance of such narratives, we may also admit a genuinely historical value, though not of the kind associated with this term in the present discussion. Traditions share with archaeological specimens, social usages, religious phenomena, and what not, the characteristic that likeness in distinct tribes calls for interpretation. Such interpretation may in many instances reveal beyond cavil, or at least indicate in a tentative way, an historical nexus otherwise unsuspected; and in such cases we are justified in speaking of an historical value of traditions, not in the sense that the traditions themselves embody truths which the ethnologist or folklorist must accept, but in the sense in which the same type of divination ritual, the same type of age-society, the same type of stone-axe, in different regions, may have an historical bearing. I will not abate one jot from this minimum historical estimation of tradition, nor will I concede an additional iota. Let us examine on what grounds such additional claims can be advanced.

Against the sceptical attitude advocated by myself a very interesting argument has been advanced, which takes us directly into the heart of the problem. "Because some traditions are manifestly unhistorical," I have been reproached, "you rashly infer that no tradition has historical validity." With some claim to credence, I may plead that the rather elementary logical con-

siderations here advanced are not entirely beyond my ken. They have nothing to do with the case, however, for this rests not on a necessarily imperfect induction, but on more general logical, psychological, and methodological principles.

That sum-total of lore which corresponds in primitive communities to what in our own culture we embrace under the headings of science and philosophy also comprises elements, in varying degrees of systematization, which are in native consciousness equivalent to what *we* call history. My general attitude towards these elements is simply this: If we do not accept aboriginal pathology as contributions to *our* pathology, if we do not accept aboriginal astronomy, biology, or physics, why should we place primitive history alone on a quite exceptional pedestal, and exalt it to a rank coördinate with that of our own historical science? This is the, to my mind, absolutely conclusive argument, which is independent of, though strengthened by, the number of cases, really tremendous, in which the glaring disparity between primitive history and our conception of the physical universe renders acceptance of tradition impossible.

The really interesting problem to me is, not what degree of importance shall be attached to so-called historical traditions, but what psychological bias could conceivably make scholars attach greater weight to aboriginal tales of migration than to aboriginal beliefs as to levitation or the origin of species. While in the nature of the case demonstration is impossible, I have a very strong suspicion that lurking behind the readiness to accept primitive for real history is the naïve unconscious assumption that somehow it is no more than fair to suppose that people know best about themselves. This assumption, of course, need only be brought up into consciousness to stand revealed in its monstrous nakedness. The psychologist does not *ask* his victim for his reaction-time, but subjects him to experimental conditions that render the required determination possible. The palaeontologist does not interrogate calculating circus-horses to ascertain their phylogeny. How can the historian beguile himself into the belief that he need only question the natives of a tribe to get at its history?

It may be objected that primitive astronomy and natural his-

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tory *do* coincide in some measure with our equivalent branches of learning, and that consequently there is a presumption in favor of the view that primitive and civilized history also overlap. To urge this is to ignore a vital aspect of the situation. We accept primitive observations of the stars or on the fauna or flora of a country as correct in so far as they conform to what we independently ascertain by our own methods. However, we neither derive the least increment of knowledge from this primitive science nor are we in the slightest measure strengthened in our convictions by such coincidence. Exactly the same principle applies to the domain of history. When a Crow tells me that his tribe and the Hidatsa have sprung from a common stock, this is correct but purely superfluous information, for I arrive at this result with absolute certainty from a linguistic comparison. In history, as everywhere else, our duty is to determine the facts objectively; if primitive notions tally with ours, so much the better for them, not for ours.

As a matter of fact, the case for primitive history is very much weaker than for primitive natural science. Natural phenomena are not only under the savage's constant observation, but a knowledge of them is of distinct importance to his material welfare. It is not strange that, say, the Plains Indians knew the habits of the buffalo, or should be conversant with the topography of their habitat. On the other hand, the facts of history are definitely removed from the sphere of observation when they have once taken place. More than that, the facts of what *we* call history are, as a rule, not facts which fall under primitive observation at all, but transcend it by their complexity and the great spans of time involved. It is as though we expected primitive man not merely to note the particular effects of rain on a hillside, but to form a conception of erosive processes on the modeling of the earth. This leads us to a point of fundamental importance.

There is all the difference in the world between correct statements of fact and historical truths. That my neighbor's cat had kittens last night may be an undeniable fact, but as a contribution to our knowledge of present-day political and social progress it is a failure. That Tom Brown moved south has one meaning

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when it suggests that he transferred his baggage from the Borough of the Bronx to a Harlem flat, and a very different one when the implication is that he, with thousands of his followers, migrated from Greenland to Patagonia. Now, my contention is briefly this: that the facts which we want to ascertain as historians are mainly of the latter order, while the facts recollected (so far as they *are* recollected) by primitive men are of the neighbor's-cat's-kittens order. In other words, I deny utterly that primitive man is endowed with historical sense or perspective: the picture he is able to give of events is like the picture of the European war as it is mirrored in the mind of an illiterate peasant reduced solely to his direct observations.

I will illustrate my contention by actual illustrations. If we examine an account by natives of events so recent that their authenticity need not be questioned, we discover what is already known to us from other fields of inquiry; viz., that the aboriginal sense of values differs fundamentally from ours. Nothing is more erroneous than to accept uncritically, say, a native statement that the ceremony of a neighboring tribe is either akin to or different from one of his own people. A trifling difference in dress may lead to an assertion of complete diversity, while a superficial resemblance may lead to a far-reaching identification. If we glance through calendar counts and Indian traditions as to actual events, nothing is more striking than the extraordinary importance assigned to trivial incidents. Such things may be absolutely true, but from none of them is the fabric of history made. On the other hand, if we turn to occurrences of tremendous cultural and historical significance, the natives ignore them or present us with a wholly misleading picture of them. Since I cannot at the present moment go through the entire literature of the subject, I will select a few instances that may fairly be taken not only as representative, but as constituting an argument *a fortiori*.

There are few events that can be regarded as equalling in importance the introduction of the horse into America; moreover, this took place within so recent a period, that trustworthy accounts of what happened might reasonably be expected. Nevertheless we find that the Nez Percé give a perfectly matter-of-

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fact but wholly erroneous account of the case,³ while the Assiniboiné connect the creation of the horse with a cosmogonic hero-myth.⁴ If we turn from the origin of the horse to the correlated phenomenon of the first appearance of the whites, corresponding facts stare us in the face. An Assiniboiné gives a tale not in the least improbable of the first meeting with whites; only the leader of the Indians at the time is said to be the culture-hero.⁵ Among the Lemhi Shoshone I failed to find any recollection of Lewis and Clark's visit, but secured a purely mythical story about a contest between Wolf (or Coyote) as the father of the Indians, and Iron-Man, the father of the whites.⁶ Do we fare any better when we turn from these representatives of a cruder culture to peoples who have attained the highest status north of Mexico? Zuñi oral tradition has it that the village at which Niza's Negro guide Estevan lost his life, and which Niza himself observed from a distance, was K'iakima. In a masterly paper Mr. F. W. Hodge has torn into shreds the arguments advanced on behalf of the aboriginal view. He establishes the fact that the village in question was Hawikuh, and that "Zuñi traditional accounts of events which occurred over three centuries ago are not worthy of consideration as historical or scientific evidence."⁷

The general conclusion is obvious: Indian tradition is historically worthless, because the occurrences, possibly real, which it retains, are of no historical significance; and because it fails to record, or to record accurately, the most momentous happenings.

This conclusion is, I am perfectly well aware, an as yet imperfect induction. To examine its ultimate validity, a special inquiry is necessary, for which I should like to outline the guiding principles.

³ H. J. Spinden, "Myths of the Nez Percé," *Journal of American Folk-Lore*, XXI (1908), 158.

⁴ R. H. Lowie, *The Assiniboiné*, AMNH, Anthropological Papers, IV (1909), 101.

⁵ *Ibid.*, Part II, p. 231.

⁶ R. H. Lowie, *The Northern Shoshone*, AMNH, Anthropological Papers, II (1909), 251 f.

⁷ F. W. Hodge, "The First Discovered City of Cibola," *American Anthropologist*, o.s., VIII (1895), 142-152.

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The historical sense of primitive peoples can be tested only by a scrutiny of *unselected* samples of their historical lore. It will not do, as some of our colleagues are wont, to reject manifestly absurd tales and to retain those which do not contravene our notions of physical possibility; for by this process we get, in the first place, a selected series of cases, and, secondly, already prejudice the whole matter by assuming that what is not ridiculously false is historically true. We must rather embrace in our survey every single statement which, whether miraculous or not from *our* point of view, is to the native psychology a matter of history. To this mass of material we must then apply our canons of trustworthiness; and from a comparison of the cases in which objective evidence supports the native statements with those in which such evidence is contradictory we may arrive at a statistically tenable attitude as to the general probability of their accuracy. Had such a test been made on unselected material, one of my critics would not have dared assert a probability of nine-tenths for native statements as to the direction from which a tribe came. In such a test as I propose, aboriginal statements that a certain tribe originated in the very spot in which it now lives must be considered exactly on the same plane as any other tradition. Similarly, all statements of a heavenly or underground origin are of equal importance, for our purpose, with any other migration legends. The fact that they are regarded as historical by the natives is decisive as to their inclusion on equal terms in any such survey as I here suggest. Now, we know that very few of our Indians could have descended from the skies or climbed from an underground world within the period of tribal differentiation of the American race; and we also know that very few of them could have arisen in the territory they now occupy, or could have occupied it for very long periods. The Yuchi, for example, have no migration legend, and consider themselves the original inhabitants of eastern Georgia and South Carolina;⁸ but we have recently been reminded that while the English colonists of 1670 refer to them

⁸ F. G. Speck, *Ethnology of the Yuchi Indians*, University of Pennsylvania, Anthropological Publications, I (1909), 8.

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as a very powerful nation, the earlier Spanish explorers between 1539 and 1567 mention no such tribe.⁹ The assumption, consequently, is that they moved into their later habitat about the latter part of the sixteenth century. This case may be taken as typical. If events dating back three hundred years are no longer recollected, we must discount the evidence of such traditional lore, and cannot accept absence of migration stories as proof of long-continued occupancy.

What, however, of the cases in which native traditions agree with objective results? The fact is simply this. The number of cardinal directions is four, or, if we include heaven and earth, six. The probability that a tribe will, in a purely mythical way, ascribe its origin to any particular one of these directions, is therefore one-fourth or one-sixth. Pending the statistical inquiries I have suggested, I wish to record emphatically the impression gained from years of experience with Indian mythology, that the proportion of historically correct statements will not be found to exceed that to be expected on the doctrine of chances.

My position, then, towards oral tradition, may be summarized as follows: It is not based, in the first instance, on a universal negative unjustifiably derived from a necessarily limited number of instances, but on the conviction that aboriginal history is only a part of that hodgepodge of aboriginal lore which embraces primitive theories of the universe generally, and that its a priori claims to greater respect on our part are nil. Such claims must be established empirically, if at all; but, so far as my experience extends, the empirical facts are diametrically opposed to such claims. The primitive tribes I know have no historical sense; and from this point of view the question whether they retain the memory of actual events, while interesting in itself, is of no moment for our present problem. The point is, not whether they recollect happenings, but whether they recollect the happenings that are historically significant. Otherwise a perfectly true statement may be as dangerous as a wholly false one. If the correct description of an excursion to a northern

⁹ J. R. Swanton and R. B. Dixon, "Primitive American History," *American Anthropologist*, XVI (October-December, 1914), 383.

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hunting-ground by part of a tribe is interpreted as the account of a permanent northern migration by the entire population, the result is wholly destructive of history.

This leads us from the field of academic discussion to that of practical work. The question that confronts the ethnological practitioner is not whether primitive history in general is trustworthy, but whether a particular aboriginal statement is correct or not. Now, what are the criteria by which its accuracy can be established? The only criterion that has ever been applied, to my knowledge, is that of physical possibility. But, as our Nez Percé illustration shows, this test is worthless: we simply shift, to use Tylor's expressive phrase, from untrue impossibilities to untrue possibilities. We know now that even trifling stories of war and quarrels are often not records of actual occurrences, but part and parcel of folk-lore, as their geographical distribution clearly shows.¹⁰ We know the force of the human tendency to mingle fancy with fact, to introduce rationalistic after-thoughts, to ignore the essential and apotheosize the trivial, not only from ethnological literature, but from a study of our civilization. Our own historical perspective is only a slowly and painfully acquired product of recent years. That like other sciences it developed ultimately from a prescientific interest in past events, that in this purely genetic sense our history is an outgrowth of primitive tradition, is beyond doubt; but, as we cannot substitute folk-etymology for philology, so we cannot substitute primitive tradition for scientific history. Our historical problems can be solved only by the objective methods of comparative ethnology, archaeology, linguistics, and physical anthropology.

¹⁰ Franz Boas, *The Eskimo of Baffin Land and Hudson Bay*, "AMNH Bull., XV (1907), 362.

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WHEN SCIENTISTS CEASED TO QUOTE FARMERS' TALES ABOUT the cleverness of their horses or dogs and devised laboratory experiments for the testing of animal behavior, a new era began to dawn in the history of psychology. Psychologists are laying aside the anecdotal method in the evaluation of individual and racial worth, and every anthropologist will welcome an improvement in technique that promises to shed light on one of the most obscure of his own problems, the question of the interrelationship of empirically observed achievement and innate capacity. Unfortunately the psychologists who are most prominently associated with anthropological applications of their new tool are so ignorant of anthropology that their results are worthless. It may be said on their behalf that they have been misled by anthropologists, that we ourselves have been guilty of spreading erroneous conceptions, but that only makes matters worse. The situation thus justifies an elementary consideration of the points at issue, a review that shall dispel the farrago of bad logic, bad biology, and bad faith that continues to pervade discussion of racial endowment.

American Anthropologist, XXV (July-September, 1923), 291-303.

ANTHROPOLOGISTS AND HEREDITY

In the first place, it may be well to repudiate some absurd misconceptions, such as the strange notion that certain anthropologists favor an extravagant influence of environmental as contrasted with hereditary factors; and that they teach the absolute equality of all races, nay of all individuals. I do not of course pretend to know the views of all living anthropologists, but I am not acquainted with any colleague who entertains these doctrines. Professor Boas is commonly mentioned as the champion of such dogmas. When, however, I turn from the garbled account of his conclusions in such works as Mr. Madison Grant's *The Passing of the Great Race* to his own statements, I find nothing to support such misrepresentation. Professor Boas argues for "a strictly limited plasticity" (*zugunsten einer eng begrenzten Plastizität*) under the influence of an altered environment.¹ On the subject of heredity he has this to say:

Although we have seen that environment, particularly domestication, has a far-reaching influence upon the bodily form of the races of man, these influences are of a quite secondary character when compared to the far-reaching influence of heredity. Even granting the greatest possible amount of influence to environment, it is readily seen that all the essential traits of man are due primarily to heredity. . . . I am inclined to believe that the influence of environment is of such a character, that, although the same race may assume a different type when removed from one environment to another, it will revert to its old type when replaced in its old environment.²

Finally, his statement as to the comparative mental make-up of Caucasians and Negroes is extremely cautious; he accepts the possibility of differences but is not convinced of such differences as would incapacitate the Negro for the exigencies of modern life.³

Personally, I take great pains to impress upon my students that the innate equality of all races is an unproved dogma, in spite of the fact that all the demonstrations of inequality hitherto at-

¹ Franz Boas, *The Mind of Primitive Man* (New York, 1911), p. 64; *idem*, *Kultur und Rasse* (Leipzig, 1911), p. 67.

² *Idem*, *The Mind of Primitive Man*, pp. 76 f.

³ *Ibid.*, pp. 271 f.

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tempted are scientifically worthless. Some time ago I formulated my views in the following words:

As to the existence of superior races, I am an agnostic open to conviction. All evolutionists admit that at some point an organic change of fundamental significance occurred. It is *conceivable* that the Bushmen and Negrito, Pygmies and Negroes are organically below the remainder of living human types, and that differences of one sort or another even divide more closely related stocks. But between what is conceivable and what is definitely established there yawns a chasm, and where the scientist has no proof he holds no dogmas, though dispassionately he may frame tentative hypotheses.

This is not a very subtle point, but seems to transcend the comprehension of some writers. One of them has even gone so far as to accuse me of denying innate *individual* differences, referring his readers to certain articles of mine that were expressly designed to illustrate these differences.

It is an interesting fact that those who most vociferously accuse anthropologists of underestimating heredity as compared with environment are themselves the worst offenders in this regard. How does President Osborn, for example, account for the differences of Cro-Magnon man in the Aurignacian and in the Magdalenian period? By the influence of environment! He writes as follows:

It is probable that in the genial climate of the Riviera these men obtained their finest development; the country was admirably protected from the cold winds of the north, refuges were abundant, and game by no means scarce to judge from the quantity of animal bones found in the caves.⁴

In the reduction of the stature of the woman to 5 feet 1 inch and of the man to 5 feet 3 inches, and in the reduction of the brain capacity to 1,500 c.cm., we may be witnessing the result of exposure to very severe climatic conditions in a race which retained its fine physical and mental characteristics only under the more genial climatic conditions of the south.⁵

This is environmentalism with a vengeance! One wonders why those who so readily account for a difference of 300 c.cm. in

⁴ H. F. Osborn, *Men of the Old Stone Age* (New York, 1915), p. 297.

⁵ *Ibid.*, p. 382.

brain capacity and of 10 inches in stature by a change in geographical conditions refuse to admit that skulls may become somewhat narrower or wider under the influence of changed conditions. The difference of 10 inches in average height is about twice as great as the difference between the Scotch and the South Italians; it is greater than the difference between Andamanese pygmies and Frenchmen; equivalent to the difference between the Nilotics and the Vedda! What does Dr. Osborn mean? Does he believe that a climatic change effected a change in the germ-plasm tantamount to a heritable mutation? Or is he merely suggesting a "modification" in Baur's sense of the term? Even on the latter assumption, he is pleading for a potency of the environment that far transcends Boas's notion of a "strictly limited plasticity."

Mr. Madison Grant is not less of an environmentalist than his scientific sponsor, but apparently he attributes precisely the opposite effects to the same climatic conditions. The Nordics, whom in the particular sections of the book I am now quoting from⁶ he is pleased to favor, are said to have developed through isolation and the selection due to the rigors of severe winters, while under "the softening influence of a life of ease and plenty" they succumb.⁷ Genial climate was necessary for the Cro-Magnons, the alleged spiritual forerunners of the Nordics, but a genial climate spells disaster for the Nordics, it seems.

Mr. Grant, however, not merely ascribes considerable influence to the environment when it so pleases him, but also implicitly denies the combined influence of both heredity and environment when the spirit so moves him. It is indeed one of his explicit cardinal doctrines that racial traits are "to all intents and purposes immutable," "fixed and rigid." He furthermore holds that in Sweden "there has been but a single racial type from the beginning" and once he even delivers himself of the statement that "Denmark, Norway and Sweden are purely

⁶ Corresponding qualifications must always be understood to accompany expositions of Grant's views, which change from chapter to chapter, and sometimes even from paragraph to paragraph.

⁷ Madison Grant, *The Passing of the Great Race* (New York, 1919), pp. 38-41, 170 f.

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Nordic.”⁸ Now we must recall that according to this author the Nordics evolved and actually flourish in the climatic conditions characteristic of their present habitation. Nevertheless he concludes a paragraph on the Scandinavian countries with this statement: “To-day all three seem to be intellectually anaemic.”⁹

As a member of the Society for the Advancement of Scandinavian Study and of the Scandinavian Club of the University of California, I venture to stigmatize this proposition as arrant nonsense. But apart from the crass ignorance it displays of the intellectual life of the peoples lampooned, how is such degeneration intelligible on Mr. Grant’s own principles? If the Nordics are by heredity a favored race; if the Scandinavians are pure Nordics; if they “flourish, do their work and raise their families”¹⁰ in precisely the type of habitat they occupy; if racial traits “do not change during the lifetime of a language or an empire”;¹¹ then, by what magical process, neither racial nor environmental, do these purest Nordics degenerate to a status of intellectual anaemia within a few brief centuries? Perhaps Mr. Grant is not, after all, the champion of heredity he professes to be when it suits his convenience.

Before leaving this writer, I will call attention to two sentences in immediate contact with each other in his chapter on “The Expansion of the Nordics.” In the first, already quoted, the three Scandinavian countries are described as “purely Nordic.” In the second, we are told that in southwestern Norway and in Denmark “there is a substantial number of short, dark round heads of Alpine affinities.”¹² Comment is superfluous.

To sum up, it is not the professional anthropologist but the professional heredity-monger that disregards the influence of heredity *ad libitum*. The anthropologist does not assert that the environment induces far-reaching effects on the germ-plasm: he merely asserts that certain phenomena change independently of the germ-plasm and in this claim he is fully supported by

⁸ *Ibid.*, pp. 15, 18, 169, 211.

⁹ *Ibid.*, p. 210.

¹⁰ *Ibid.*, p. 39.

¹¹ *Ibid.*, p. 15.

¹² *Ibid.*, p. 211.

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the attitude of Professor Elliot Smith, one of the few scientists with primarily biological orientation who have not disdained to try to understand the meaning of culture.¹³

INNATE ABILITY AND CULTURE

In the past, arguments on racial differences have almost always been advanced on the assumption that observed differences in cultural achievement must be the expression of correlated differences in inborn capacity. In one sense no one denies this; everyone would admit that a cat, a dog, or a monkey is incapable of producing or sharing in human culture. The point at issue is whether, when the organization adequate to the production of culture or, let us say, of the culture characteristic of the Upper Palaeolithic was reached, any further cultural advance was conditioned by equivalent changes in inborn equipment. The differences between the material culture of, say, the West African Negro or the Shoshoni of Idaho on the one hand and Western civilization on the other are so striking that most writers naïvely assume that they are patent proofs of organic differences, and popular prejudice doubtless rests on the same fallacy.

The argument is fallacious, in spite of its plausibility, for the following reason. When we study the known history of culture, we find great changes *without* any corresponding changes in racial constitution. In 1850 no one dreamt of crediting the Germans or the Japanese people with efficiency. Elizabethan England was very different from the England of Queen Anne's day; and those who talk as though an aversion to discussions of sex were a deep-rooted Anglo-Saxon trait have perhaps slight acquaintance with Fielding and the Restoration dramatists. It is true that Galton asserted a racial cause for the magnificence and the decline of Athenian culture, but his claim is an empty allegation and contradictory to his own interpretation of the Renaissance.

The instances hitherto cited involve, however, relatively slight differences when viewed in broadest perspective. Hence it seems desirable to supplement them by others. It is not merely admitted but contended that the Nordic race has not changed in inborn

¹³ G. Elliot Smith, "Primitive Man," *Proceedings of the British Academy*, VII (1916), 37, 49 f.

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equipment for several thousand years except in so far as it has been debased by amalgamation with inferior types. Yet the culture of the Nordics has developed extraordinarily within the space of from two to three thousand years. The Cro-Magnons provide an even better illustration. They appeared about, say, 25,000 B.C. and persisted through Magdalenian times, which began about 16,000 B.C.¹⁴ Here we have a race at least originally superior in inborn capacity to any now living, yet in 9,000 years or more they cannot rise above the level of the Stone Age culturally! Nay, the case is still more curious, for it is the *decadent* Cro-Magnons—short and with reduced brain capacity—who achieve the triumphs of Palaeolithic art!

Culture evidently does not vary with race according to any simple formula of functional relationship. This does *not* prove that the Tasmanians or Bushmen or Andamanese had the inborn capacity to develop unaided the civilization of Western Europe. It does prove that the difference of their culture from ours is not necessarily rooted in any innate difference, that the popular argument is wholly inconclusive. We simply do not know whether the evolution of *Homo sapiens* involved all the organic requirements for any type of culture known, or whether certain deficiencies, as yet undefinable, necessarily bar certain varieties of the species from independently attaining such and such a cultural status.

Since, then, the gross comparison of cultural achievement leads nowhere, so far as the determination of innate possibilities goes, let us turn for aid to the psychologist. Here, too, however, certain elementary precautions are prerequisite.

PRELIMINARY CONSIDERATIONS

A comparison of distinct *groups* involves the consideration of both average values and variability. It is entirely conceivable that two groups should coincide in their average mentality but differ in range, so that one may produce far more remarkable individuals in both positive and negative direction than the other. Professor Fischer, for example, suggests that the Caucasian differs from the Negroid in precisely this point, while not excel-

¹⁴ Osborn, *op. cit.*, pp. 18, 261, 351.

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ling him in average intelligence. If this could be established, it would have far-reaching theoretical and practical bearings: it would account for the differences in cultural achievement without assuming that the *average* level of intelligence varies in different cultures; and it would imply that for the ordinary tasks of life the Negroid is as well fitted as the average white.

In connection with the occurrence of extreme positive variations it is well to bear in mind another point forcibly made by Father Wilhelm Schmidt. Extreme deviations from the norm naturally occur with greater frequency in large populations than in communities of several hundred. A class of fifty may have the average stature of the whole student body, but it is not so likely to have as tall members as occur in the total campus population of, say, ten thousand. It is not astonishing, then, that hordes of Andamanese or Australians numbering not over a hundred or two should never have produced the personalities which figure in the history of China, India, and Western countries.

Another caution is of tremendous importance. Since we are interested in establishing the existence or non-existence of *innate* differences, the influence of training and other noncongenital factors, all of which for convenience' sake we may call environmental, must be eliminated. The lightheartedness, not to say unscrupulousness, of many writers on this point is appalling. Admitting, as they must, that an empirical test cannot eliminate the environmental factor, they decree that certain observed differences are too great to be explained by environmental differences, hence are evidence of hereditary differences. The illegitimacy of this reasoning is apparent as soon as it is couched in clear language. Letting H and E represent hereditary and environmental determinants, respectively, the empirical results may be formulated as follows:

$$\begin{aligned}H_1 + E_1 &= A \\H_2 + E_2 &= A \pm m\end{aligned}$$

It does not require a profound knowledge of mathematics to see that the difference $\pm m$ proves nothing as to the value of H_1 and H_2 so long as E_1 and E_2 differ by an unknown quantity. This is not academic logic-chopping pure and simple: we are

told that Negroes are inferior to Caucasians because in certain tests 79 per cent of the former fell below C as against 25 per cent of Caucasians while only 1 per cent of the Negroes as against 12 per cent of the Caucasians scored above C. This difference, we are told, is too great to be interpreted as the result of educational and other social differences. But New York Negroes practically equal Alabama whites in the tests! Hence the environmental factor *must* be taken into account, and unless we devise accurate methods for its quantitative determination, let us hold our tongues concerning inborn differences.

RACIAL AND NATIONAL GROUPS

It is a commonplace of modern science that racial and national groups rarely coincide. This has not deterred several prominent psychologists from blandly grouping immigrants into the United States according to their place of origin and then proclaiming that the results of the ensuing group tests are *racial* statistics. This is the well-nigh incredible procedure of Dr. Robert M. Yerkes in an article on "Testing the Human Mind," contributed to the *Atlantic Monthly* for March, 1923. Dr. Yerkes not only brushes aside in cavalier fashion the educational differences discussed in the preceding paragraphs but cites tests on Italians, Poles, Turks, Greeks, *et al.* as establishing *racial* differences. He also ingeniously suggests that the Mediterranean element accounts for the low scores of recent immigrant groups; that element apparently possesses the miraculous quality of detracting from the Italian average by its presence and from the Polish average by its absence.

I wonder what would be thought of a naturalist who should wish to ascertain the characteristic weight of pure breeds of dogs by averaging an odd assortment of St. Bernards, dachshunds, and bulldogs and comparing the result with a corresponding average for mastiffs, fox terriers, and German police dogs. As a humble exercise in arithmetic the procedure may be justified, but its biological significance would be nil. Yet it would be better than Dr. Yerkes's method, for at least the naturalist would know precisely how many individuals of each breed he had weighed, but when Dr. Yerkes tests "Italians" he does not know how many

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of them represent each of the relatively pure types whose inborn endowments he is attempting to ascertain.

At this point I must register an emphatic protest against the naïve assumption that because certain individuals in a region in which mixture of types has demonstrably occurred display *physical* features characteristic of type A they are therefore likewise the possessors of the mental traits that are *ex hypothesi* distinctive of the primeval "pure" type A. President Osborn goes further and lays down the proposition that even when one of the most typical traits of the Nordic, blondness, is lacking the individual may still be "three-fourths or seven-eighths Nordic, because it only requires a single dark-eyed ancestor to lend the dark hair and eye color to an otherwise pure Nordic strain."¹⁵ By implication dark hair and eye color will be the only features to dominate and the psychological traits of courage, loyalty, self-sacrifice and idealism innate in the Nordic will remain dominant in miscegenation. There is of course not a shred of evidence in support of such a principle of inheritance. One might well despair of modern biology if such slovenly pronunciamientos were not rejected by sane students of the subject. As Doctors East and Jones point out, we must be

very cautious about drawing genetic conclusions in the human race based upon the possession of particular traits, in the absence of proof of a long-continued isolation. . . . Traits originally characteristic of certain peoples because of isolation and the consequent inbreeding have been shifted back and forth, combined and recombined. . . . It is wholly possible, for example, that a tall, blue-eyed, dolichocephalic Frenchman really possesses less of the so-called Nordic factors than a short, dark-eyed round-head.¹⁶

Two other points may well be emphasized in this context. For one thing, the variability of "pure" types is largely unknown; we do not know, for example, how probable it is for a "pure" Alpine to vary so much from the norm of his type as to appear like a typical "pure" Nordic. Secondly, it is about time for writers on European anthropology to realize that things are more com-

¹⁵ H. F. Osborn in "Preface to Second Edition" of Grant, *op. cit.*, pp. xi f.

¹⁶ E. M. East and D. F. Jones, *Inbreeding and Outbreeding* (Philadelphia and London, 1919), p. 250.

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plicated than a hasty perusal of Ripley's book, now twenty years old, may indicate. Apart from the Adriatic or Dinaric race recognized by many investigators, we may have other types to consider if Dr. Czekanowski and other anthropologists are correct in their observations in Poland and Russia.¹⁷

PROGRAM

Is it, then, necessary to abandon all hope of progress in this field? By no means: a calm survey of the difficulties merely leads to a formulation that does not by necessity produce absurd and worthless results. We cannot hope to eliminate all disturbing factors, but that is equally true even of such ancient sciences as astronomy. We can at least get rid of certain conditions that are bound to vitiate comparative results.

First of all we must choose a region that is anthropologically well known and which has been demonstrably occupied by more than one racial strain, but in which strains are locally more or less segregated. Without assuming that it is the only country suitable for the purpose, I venture to suggest that Italy provides a very favorable starting-point. The contrast between the North Italian Alpine type and the South Italian Mediterranean type is notorious. While of course minor variations are not lacking in the south, the uniformity of the South Italian population is remarkable.¹⁸ The hair is almost always black; the nasal index for Abruzzi, Campania, Puglie, and Sardinia is 69.77, 69.68, 69.49, and 68.82, respectively; the stature ranges provincially between the narrow limits of 159.9 cm. for Basilicata to 162 cm. for Campania; "mixed brown" pigmentation occurs in at least half of the individuals examined, rising to 62.2 per cent in Calabria and 70.4 per cent in Sardinia. When we consider, on the other hand, such typical North Italians as the Piedmontese and Venetians, we discover that the hair is often, if not almost always, of chestnut color; that the mean height is distinctly greater than among the Mediterraneans—166.3 against 163.7 cm.; that there is an ap-

¹⁷ Jan Czekanowski, "Recherches anthropologiques de la Pologne," Société d'Anthropologie de Paris, *Bulletins et Mémoires*, ser. 7, Vol. I (1920), 48 ff.

¹⁸ For the data that follow see V. Giuffrida-Ruggeri, "A Sketch of the Anthropology of Italy," *RAI, Journal*, XLVIII (1918), 80-102.

preciable percentage of individuals with fair pigmentation. In addition there is the marked difference in head form: the Piedmontese with an index of 85.7 and the Venetians with an index of 85 are markedly brachycephalic; the South Italians, while contrary to current statements *not* dolichocephalic at present, are either mesocephalic or merely of moderately brachycephalic character. Nevertheless, when we compare the head form of the several South Italian provinces, the impression of homogeneity so strongly suggested by other physical traits disappears; between the extremes represented by Sardinia with 77.5 and Campania with 82.1 there are intermediate figures, such as 78.4 for Calabria and 80.8 for Basilicata.

These data furnish us with the possibility of sketching a program for psychological investigation. In the first place, it is probably not difficult to minimize the environmental factors: a thousand illiterate peasants from Sardinia will probably not differ notably in their cultural influences from an equal number of illiterate peasants from Sicily. Secondly, when we find such regional differences in head form within an otherwise uniform population, they can plausibly be accounted for through racial mixture; specifically, the relatively broad-skulled groups are presumably such through the influence of Alpine mixture. The alleged innate mental differences are accordingly amenable to empirical verification or disproof: the Calabrians with an index of 78.4 may be assumed to be more like the Basilicatans (80.8) than like the people from Abruzzi (81.9) and Campania (82.1); the Sicilians (79.6) will be more like the Apulians (79.8) than like the other groups mentioned. I am well aware of the fact that very small differences, possibly derived from small series, may not be significant. It is also obvious that, with the variety of complicating factors, the ideal of quantitative refinement here outlined cannot be realized. Nevertheless, if there is anything in the alleged mental difference of the Alpine and Mediterranean types, the *repeated* comparison of all the otherwise homogeneous Mediterranean groups differing only by a varying degree of Alpine admixture indicated by the cephalic index should constitute a crucial test. In Sardinia, with its excessively dark pigmentation, relatively greatest degree of dolichocephaly among the living

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(77.5), genuine dolichocephaly (71.53) of cranial material, and maximum trend toward curly hair and prognathism, an especially favorable opportunity presents itself for ascertaining the psychological influence of the Negroid strain that has plausibly been assumed as the factor determining these deviations from the South Italian norm. In the north, the aberrant case of Liguria, where the index of 79.34 stands out in marked contrast with that of the neighboring brachycephalic provinces, corresponding comparative tests seem desirable.

While I have stressed the cephalic index in view of Italian conditions, I should not like to be interpreted as disregarding other physical traits. In Portugal, for example, it may well be that the regional distribution of blondness would provide a better line of cleavage than the character of the head form.

A sane procedure will involve the systematic exploitation of *minimal* differences in conjunction with historical data. The Danes are known to have had largely the same antecedents as the other Scandinavians but they are about three centimeters shorter and have an index of 80.7 as against 78.5 for Norway. To what extent do they differ in mental make-up from other Scandinavians? In Norway a number of interesting problems arise. In sections of the country where no Lapps are known ever to have existed there is a marked percentage of dark-eyed people.¹⁹ This locally segregated group invites comparison with their typical blue-eyed "Nordic" neighbors. The latter may be compared with those Norwegian groups which have demonstrably intermarried with Lapps. Again, "pure" Lapps, such as those measured by Mantegazza, have an index over 87, while the "Lapps" of Troms, where mixture has occurred, have an index of 84.3, besides differing in other respects. Finally, the Karelian Finns differ appreciably from the Finns proper and might well be psychologically tested in comparison with them.²⁰ If I remember Professor Retzius' statement correctly—his volume is not accessible to me at present—the history of the Walloons imported into Sweden is fairly well known, and certain districts still clearly reveal the infusion of Alpine blood. Here, then, a

¹⁹ Halfdan Bryn, *Troms Fylkes Antropologi* (Christiania, 1922), p. 19.

²⁰ *Ibid.*, pp. 33, 37, 174.

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comparison of Alpine and Nordic mentality may be feasible.

No doubt many readers of this journal can suggest additional problems. When psychologists without bias shall have attacked them and arrived at statistically unexceptionable positive results, i.e., shall have established real innate differences, anthropologists will accept the conclusions regardless of their personal predilections or prejudices. In the meantime it is their duty to denounce the charlatanism so prevalent in this field and to repudiate not biology but the sham biology that invents facts and even biological "laws" to support personal views.

Incorporeal Property in Primitive Society

AMONG THE PROBLEMS THAT EXERCISED THE MINDS OF THE earlier evolutionists who dealt with human society, that of property was one of the most important. Its influence in modern industrial civilization was potent; hence the evolutionary schematist naturally assumed that in the earliest phases of culture it had been nil. Lewis H. Morgan's views may be taken as representative. He distinguished three major periods,—Savagery, Barbarism, and Civilization. The beginnings of Barbarism were defined by the invention of pottery, those of Civilization by the use of a phonetic alphabet and literary records. The two former periods were subdivided each into a Lower, Middle, and Upper Status. It was not until the Middle Status of Barbarism—exemplified by the village life of our Southwestern Indians, of the aboriginal Mexicans and the Peruvians—that Morgan assumed property to have played an important part. Among "savages," he held, property was inconsiderable.

Their ideas concerning its value, its desirability and its inheritance were feeble. Rude weapons, fabrics, utensils, apparel, imple-

Yale Law Journal, XXXVII, No. 5 (March, 1928), 551-563.

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ments of flint, stone and bone, and personal ornaments represent the chief items of property in savage life. A passion for its possession had scarcely been formed in their minds, because the thing itself scarcely existed.¹

In short, Morgan does not deny that pre-ceramic savages had chattels, but he minimizes the importance of the property held, and of the correlated acquisitive urge. His successors have generally followed his leadership and assumed as a matter of course that on primitive levels property rights were weakly developed, there being a far-reaching communistic trend; and, specifically, that land was not appropriated by individuals or even families in the hunting stage.

These propositions are no longer tenable. In part they rest on ignorance of the ethnographic data, often on a failure to discriminate between moral and legal prescriptions. To illustrate the latter point, it is unquestionably customary to share the necessities of life in a manner that sometimes amounts to practical communism; yet, as a rule, in strict aboriginal law the line is clearly drawn between what is one's actual due and what is merely an ethical claim. There are, indeed, extreme instances. In northeastern Siberia a boat lying idle may be put to effective use without the "owner's" consent, nor is the borrower liable for damages in case of injury. Yet among these same populations, other forms of property are jealously guarded from encroachment. As for land, Seligmann has shown that the Vedda of Ceylon not only own tracts individually but practice a form of conveyance; and the prominence of hunting-territories among our Northeastern Algonkians of New England and Eastern Canada has been extensively described by Professor Speck.²

This position is fully borne out by data on two genuinely "savage" groups, in Morgan's sense—the Yamana (Yaghan), the most southerly of South American tribes, and the Semang, a Negrito people of the Malay Peninsula.

The Yamana, in particular, exemplify the mingling of ethical and legal principles that has sometimes in the past misled sociological interpretation. Here there does not happen to be

¹ L. H. Morgan, *Ancient Society* (New York, 1877), Part IV, chap. 1.

² R. H. Lowie, *Primitive Society* (New York, 1920), chap. ix.

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individual or family ownership of an economically valuable area, which is held to belong to the entire territorial group. Certain raw materials, such as iron pyrites for fire-making and a species of tree whose bark was suitable for the native types of canoe, were restricted to definite localities; and, in these instances, utilization was permitted to territorial groups other than those within whose normal range these natural resources happened to lie. Nevertheless, personal property rights were recognized and, as usual, they rested on individual manufacture and effective use. Baskets must be bought from women, harpoons from men; as elsewhere in North and South America, even the children's claims to ownership are respected. While most of these chattels were burnt with the corpse, a dog was invariably inherited by the eldest son or some other kinsman or acquaintance. Food is treated in the quasi-communistic fashion often reported for primitive tribes. In a particular case a successful seal-hunter immediately divided his kill into seven portions, of which he retained two, dividing the remainder among the five tribesmen present. Similarly, it is considered self-evident that the discoverer of a stranded whale should not play the part of a miser but should forthwith spread the glad tidings. Yet it is interesting to note that he had a prior claim to the booty and might select favored pieces or direct the distribution. However, no one was privileged simply to appropriate his neighbor's food, and any one who abused the privilege of hospitality soon fell in public estimation. There was also a pronounced tendency to make presents, whether of food, necklaces, slings, spears, or other implements; and acceptance involved the obligation of making a suitable return gift. The very fact of this institution constitutes proof of individual property rights.³

For the Semang the same general principle holds. Clothing and tools are personal property and can be borrowed only with the owner's consent. Husband and wife pool their possessions without relinquishing their separate claims, and neither spouse inherits from the other. The hut belongs to its normal builder, i.e., the woman, so that a divorced husband is obliged to leave it.

³ Wilhelm Koppers, *Die Formen des Eigentums der Yamana auf Feuerland* (1926), 3 Neue Ordnung, pp. 1-22.

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Food is, indeed, shared with fellow-tribesmen, at least so far as they are related. However, two species of trees, the durian fruit-tree and the ipoh, which furnishes arrow-poison, are owned individually. To every adult male belong one or more ipohs and several scattered durian trees. No one would venture to trespass on these prerogatives by cutting into an ipoh or climbing a durian trunk.⁴

The dogma of general primitive communism is, however, at once eliminated by the wide prevalence of individually owned forms of incorporeal property. Their very existence—sometimes on very rude levels, indeed, and alongside of virtual communism in other directions, is a noteworthy phenomenon; and the restrictions on *absolute* ownership rights imposed by the varying mores of different peoples are no less interesting.

The Eskimo may be profitably studied from this point of view. Like the Arctic Siberians', their hunting customs, viewed in isolation, might go far to support the notion that personal ownership is lacking. Yet the magical formulae that secure the Central Eskimo's luck in the chase are not shared communally. One man who was very successful in catching salmon stated that his grandmother had taught him what to sing when fishing. This song for salmon is also effective for seal; but for ground-seal he must sing another one, and still others for musk-oxen and for caribou. He had not taught these songs to his children, but intended to do so before he died. Nor were incantations confined to hunting: anciently people could use them to shorten their journeys, but while reciting a spell of this category they were not allowed to look back.⁵

Fuller data are available for the corresponding phenomena among the Greenland Eskimo. Spells are emphatically private property (*en privat og hemmelig ejendom, andre ikke maa bruge*). They are potent not because of any spiritual agency but through the virtue of the words themselves, even though these are sometimes unintelligible. Some of the spells correspond to household remedies that eliminate the need for calling upon

⁴ Paul Schebesta, *Bei den Urwaldzwerge von Malaya* (1927), pp. 78 ff., 225.

⁵ Franz Boas, *The Eskimo of Baffin Land and Hudson Bay*, AMNH Bull., XV (1907), 153, 506.

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a shaman to treat a patient. The largest number, however, are designed to secure good luck in the chase. To quote Holm:

The charms are of great antiquity, and are as a general rule handed down from one generation to the other by sale. They are most effective the first time they are used and little by little they lose their power; hence they must not be used except in times of danger, or when they are transferred to another. When the transference takes place, none but the buyer and seller may be present, and in order that they may have effect, they must be paid immediately, and dearly paid too, if there is to be any power in them; but then they do the possessor much benefit. The payment may consist, *e.g.*, of dart points, lance points, or other costly iron work. As they are much reluctant to use the charms without absolute necessity, it is extremely difficult to get to hear them.

One of Holm's informants recited for him a charm he had effectively used when on the point of death. The explorer says: "I paid for hearing it, otherwise it would have lost its power."⁶

The inconsistency involved in the Eskimo position has already been alluded to: a communistic trend as to economic necessities is coupled with strict individualism as to the magical means of securing food. Let us also note the limitations imposed. Effectiveness, for one thing, is contingent on purchase: in other words, the owner is not *absolute* owner in a metaphysical sense, for he cannot give away his spell as a gift without destroying its efficacy—a rather transparent rationalization. How far this view applies to the Central Eskimo instances is doubtful; possibly there the children have a preëptive claim to instruction in their elders' sacred knowledge.

Among the Arctic Northeast Siberians similar conceptions hold sway. The incantations sung by the Koryak are derived ultimately from the Creator and have a variety of virtues,—curative, game-luring, and what not. They are usually in the custody of elderly women, who do not lightly divulge their sacred knowledge lest its efficacy be destroyed. A statement of Jochelson's concerning the conveyance of ownership is most illuminating: "When a woman sells an incantation, she must promise that she gives it

⁶ W. C. Thalbitzer, *The Ammassalik Eskimo* (Copenhagen, 1914), pp. 87 ff., 305; *ibid.* (1923), pp. 248–278; *idem*, *Eskimoernes Kultiske Guddomme* (Copenhagen, 1926), p. 34.

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up entirely, and that the buyer will become the only possessor of its mysterious power." In other words, acquisition of full proprietary rights involves more even than the esoteric formula; it requires also the transfer of a rough equivalent of "good-will."⁷

This naturally leads to a vindication of the incorporeal character of certain forms of property that at first blush do not appear to merit that designation. For example, a superficial view of the ceremonial complexes commonly transferred among the Plains Indians would emphasize the material contents conveyed. To make the matter concrete, the Blackfoot (Montana, Alberta) had a series of so-called military societies, each of which was entered jointly by a group of approximate age-mates who purchased membership outright from an older group. Thus, the Hidatsa Dog organization of any period comprised individuals all of whom had collectively bought such badges as eagle-bone whistles, owl-feather headdresses, and dewclaw rattles from the company preceding them as owners of the ceremonial complex labeled "Dog." In so far as all these and other regalia were tangible objects, the term "incorporeal property" might be challenged in this connection. However, closer scrutiny reveals the fact that a transfer implied much more than a mere purchase of the ordinary character. First of all, the buyers obtained the right to perform a specific dance, and some of their officers gained the prerogative of appropriating any food suspended from the meat-racks in the camp. But even the badges themselves were not prized in themselves but in their proper setting: a dewclaw rattle, for example, could have been imitated by the Kit-fox or Lumpwood society, but unless duly bought in the approved fashion from the rightful owners it was nothing but a travesty of the real article.⁸

This point of view appears still more clearly in the case of the sacrosanct complexes known as sacred bundles. The Beaver bundle of the Blackfoot comprises an amazing variety of disparate objects, such as skins of beavers, muskrats, and wildcats;

⁷ Waldemar Jochelson, *Material Culture and Social Organization of the Koryak*, AMNH, Memoirs, X (1908), 59.

⁸ R. H. Lowie, *Societies of the Crow, Hidatsa and Mandan Indians*, AMNH, Anthropological Papers, XI (1913), 225 ff.

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skins of various birds; and so forth. Naturally, it would not be difficult to duplicate these elements, but it is not in them that the mystic potency of the Beaver bundle inheres; they are nothing but outward symbols of what is essential, to wit, the privilege to sing certain sacred songs and to perform the ritual associated with the objects.

"At the formal transfer, the ritual is demonstrated as far as possible, four days and nights being required to complete it. In the normal order of events the ex-owner continues to instruct the purchaser for an indefinite period."⁹

Unless a man had received this instruction he would not own anything genuinely valuable. In other words, he buys a series of prerogatives including one or more songs, the right to certain specific modes of behavior, knowledge of the origin myth connected with the bundle, and a tangible object or set of objects within a wrapping, to be guarded and opened according to certain rules.

Why unsanctioned mimicry of the material parts of the bundle would be futile, becomes at once obvious from an exposition of aboriginal theory. The Blackfoot believe that every bundle emanates from a direct revelation by a supernatural power.

The being appearing in the dream offers or consents upon request to give power for some specific purpose. This is done with more or less ceremony; usually the face and hands of the recipient are painted, songs sung, directions given for invoking the power and certain obligations, or taboos, laid upon the recipient. The being conferring power is not content with saying that it shall be, but formally transfers it to the recipient with appropriate ceremonies. This is regarded as a compact between the recipient and the being then manifest, and each is expected to fulfill faithfully his own obligations.

Whenever the ritual is performed, it is supposed to be a faithful replica of the initial transfer. One of the significant phenomena in this whole affair is the original visionary's right to transfer the contract to another, who thus acquires all his predecessor's rights. Only by this quasi-apostolic succession can the *rapport*

⁹ Clark Wissler, *Ceremonial Bundles of the Blackfoot Indians*, AMNH, Anthropological Papers, VII (1912), 100, 107, 168 ff., 272 ff.

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with the supernatural world be maintained; hence an invasion of copyright would not help insure the blessings—longevity, health, and happiness—linked with authorized ownership. On the other hand, the genuine proprietor cannot lose the benefits connected with a bundle: “the bundle may be lost or destroyed without seriously damaging the owner, since he owns the ritual which is immaterial.” Indeed, certain advantages cling to a former owner even after he has divested himself of his formal privileges. He may be called upon to officiate in a ceremony because of his recognized familiarity with it, or may administer a deceased owner’s bundle until a proper transfer is consummated, and in either case would pocket a fee.

As regards the transmission of sacred bundles, basic differences divide the Plains tribes. With the Omaha and Pawnee, inheritance by the next of kin takes the place of the Blackfoot notion of transfer. While the Blackfoot do not exclude a son from accession to his father’s ceremonial privileges, he must acquire them like any stranger, i.e., by the same formal acts of conveyance, and these more frequently obtain between unrelated tribesmen, though symbolically the purchaser is regarded as the “son” of the seller. An interesting fusion of the two contradictory principles encountered in the area occurs among the Hidatsa. They, unlike the Blackfoot, had sons and daughters regularly acquire a bundle from their own father, but they must invariably pay him for it, one of the brothers afterwards becoming its custodian. In other words, children inherit the right of jointly buying proprietary rights from their own father. The latter retains the privilege of joining in the ritual activities, of singing the songs and offering prayers during a performance. Bound up with each bundle are a host of specific prerogatives, such as using a particular method of painting some object in the bundle. These are purchased on the same occasion as the bundle, but must be paid for separately. “A privilege of this sort may be sold four times by the owner, whereupon he loses all his title to it, as among the Crow in corresponding cases.” Here, as among the Blackfoot, the *spiritual* transfer was essential. In fact, the buyer usually did not get the identical objective constituents of his father’s bundle but sought to *duplicate* them by requisitioning

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the services of a father's clansman; only if the latter failed in his quest did the father supply what was necessary.

"It was the immaterial proprietary rights to a bundle and its ritual that were established by the transfer ceremony, which transformed a potential into an actual prerogative."¹⁰

The data for the Northern Plains tribes invite nice discussions of the basic character of "ownership" in this connection. On the one hand, it would appear a priori that where property rights are directly conferred by divine or supernatural agency they must be ipso facto indefeasible. That is, of course, true with reference to *human* instrumentalities. Full knowledge of the rituals is monopolized by the owner, and any one else speaking about them from observation or hearsay is not only limited in his information and almost bound to fall into error, but stands revealed as a poacher encroaching on an alien preserve. However, the supernatural origin of the power held implied its revocability by the source of the blessing. Specifically, any infraction of the rules linked with the bundle was fraught with danger. For example, a Beaver bundle imposed many and onerous restrictions on its Blackfoot possessor. If he comes to the bank of a stream he must not turn back but must cross since he is not supposed to show fear of water in any form. Cooking must never be done outside his tipi, yet its sides may not be lifted, irrespective of the temperature. He must never blow the fire; in case of necessity he is allowed to blow through a pipestem. He cannot take back property borrowed from him. He may not eat of the beaver nor of the birds in the sacred pack. "The narrator," writes Wissler, "was once up in the mountains and was greatly famished. Finally he ventured to eat a grouse. This made him deathly ill." In short, the owner of sacred property is in many respects not its master but its slave.

Another limitation of full ownership has already been mentioned: the owner frequently is not empowered to give his sacred privileges away, he can only sell them, even though it be to his own son or daughter. Thus, the designs painted on members in the initiation ceremony of the Crow Indian Tobacco

¹⁰ R. H. Lowie, *Sun Dance of the Shoshoni, Ute, and Hidatsa*, AMNH, Anthropological Papers, XVI (1919), 415 ff.

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society were not free for all; each method of decoration represented a prerogative acquired in a specific vision, and was transferable in the usual manner of ceremonial rights.

"Greybull . . . had once acquired the painting privilege from his own mother, paying her an ermine shirt, a horse, quilts, and money. He sold the right to Plenty-coups for four horses."¹¹

Perhaps still more significant is the fact that a bundle owner may be forced to part with his property. In some cases the obligation is apparently moral rather than legal. Thus, Strikes-at-night, a Crow woman impoverished by her husband's blindness, coveted a Horse Dance bundle supposed to bring good luck. Direct offers of purchase were declined by the owners. To quote my informant,

"One owner wanted a tent and needed hides tanned. Since I was a good tanner, I went to his wife and offered to tan all the requisite hides without demanding pay outright. I got two hides the first time; they were large. I fixed them nicely, returned them, and said I would fix up the whole lodge for them."

In this way Strikes-at-night prepared all the hides. Then the woman favored asked what pay the tanner wanted. "No, I want to take your medicine." The beneficiary said, "If you had told me before, I should never have let you finish the hides. Now I can hardly refuse you." Accordingly, she and her husband adopted my informant, conveying the ownership to her. "The other people were telling me I was very cunning because of the way I got the medicine."¹²

In the instance just cited the impression conveyed to me was that the compulsion, however strong in a moral sense, was not complete. That is to say, my informant's adopter might have been charged with ungraciousness had she refused to accede to the tanner's demands, but could not have been coerced into acquiescence. But among the Blackfoot the situation was different. A man in dire need might make a vow that if he came out of his difficulty safely he would buy a particular type of bundle.

"Such appeals are usually made to the sun. The vow usually

¹¹ *Idem*, *The Tobacco Society of the Crow Indians*, AMNH, Anthropological Papers, XXI (1919), 149.

¹² *Idem*, *Minor Ceremonies of the Crow Indians*, AMNH, Anthropological Papers, XXI (1924), 331.

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names a particular bundle and is registered before witnesses. In such cases, the owner has no option, to sell being imperative.”¹³

What results, then, from a survey of ceremonial rights in the Plains is that some of them are unequivocally personal. That is to say, they are not shared even by the next of kin nor do they automatically accrue to the holder's heirs. In this respect, a difference obtains between them and otherwise comparable privileges among the Nootka of Vancouver Island. Here each family has its stock of songs, “no outsider being permitted to make use of them, unless deputed to do so by the owner. . . . Any woman may be hired to sing her . . . song at a menstrual potlatch, being paid for her services by the giver of the ceremony.”

While *some* privileges are individual in the sense that they might be withheld from the normal heir under unusual circumstances, the most characteristic ones could not be diverted from the eldest-born son in this region of aboriginal primogeniture, so that the lineage of eldest-born descendants virtually constitutes a joint-company as regards the relevant rights.¹⁴ Notwithstanding, however, the individual nature of ceremonial ownership in the Plains, the religious and ethical notions bound up with it materially limit full property rights, sometimes even in a definitely legal sense.

Equally instructive are the data from the Trobriand Islands off the east coast of New Guinea.¹⁵ In his eagerness to emphasize the distinctive types of ownership in different parts of the world, Dr. Malinowski goes so far as to regard it as “a grave error to use the word ownership with the very definite connotation given to it in our own society.” Because the meaning we attach to it is linked with highly developed economic and legal conditions he infers that “therefore the term ‘own’ as we use it is meaningless when applied to a native society.” Worse than that, it “smuggles a number of preconceived ideas into our descrip-

¹³ Wissler, *op. cit.*, pp. 155, 174.

¹⁴ Edward Sapir, personal communication; also, *idem*, “A Girl's Puberty Ceremony among the Nootka Indians,” *Transactions of the Royal Society of Canada*, 3d ser., VII (1913), 67–80.

¹⁵ Bronislaw Malinowski, *Argonauts of the Western Pacific* (London, 1922), pp. 81–104, 116–120.

tion." This, however, is manifestly to exaggerate a legitimate point. To be forearmed against the perils of loosely applying our terminology, to be keenly sensible of the uniqueness of any particular society and its institutions, is an excellent thing. But we cannot coin a special word for every shade of possessory right as locally defined in the four quarters of the globe. It is far more important to define all such rights conceptually than to devise an infinite series of labels for them, a demand logically implied in Dr. Malinowski's contention, though his common sense prevents him from conforming to it.

His discussion of the sociology of canoe ownership, which immediately follows the propositions cited, furnishes excellent illustrations both of his point and our qualification of it. Dr. Malinowski demonstrates conclusively that the *toli-waga* or "canoe-owner," to use the nearest English equivalent, is not an absolute owner. While he has the right to choose or eliminate his companions on an expedition, his maternal kinsmen "have, according to all native ideas of right and law, a strong claim on the canoe." Further, even unrelated patricians of the community could not easily be excluded in the absence of special cause; and still others would have a moral "de facto right to sail" because of their skill as mariners. Again, it is the *toli-waga* that assembles the council and broaches the question as to the date of sailing. However, this right of initiative, on closer scrutiny, turns out to be purely nominal, since "both in construction and sailing, the date of enterprise is determined by outward causes, such as reciprocity to overseas tribes, seasons, customs, etc." This same sort of relative property right is distinctive of the "Kula," an extraordinary system of exchange by which armshells and necklaces ceremonially prized are exchanged, each gift being repaid by an equivalent counter-gift. Here, Dr. Malinowski points out that the recipient never retains his acquisition for any length of time,—never for more than a year or two, and even this moderate period "exposes him to the reproach of being niggardly." In other words, public sentiment demands that valuables of this type be kept in circulation. Evidently our authority is warranted in saying that this sort of "ownership," esteemed because of the renown coupled with even fleeting possession, is *sui generis*.

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Notwithstanding this admission, however, there is evidence that ownership quite as complete as any found in any community coexists with those more limited forms that have so deeply impressed their reporter. Let us return to the *tolu-waga*. However he may be restricted in the practical utilization of a boat, the honorific title is indefeasibly his. Even when his closest maternal kinsmen collectively apply it to themselves, "this would be an abuse of the term." Further, "the mere privilege of using exclusively this title is very highly valued by the natives"; and though the right of summoning the council and inaugurating a voyage is admittedly nominal, "the formal privilege is strictly confined to the *tolu-waga*, and highly valued," quite apart evidently from the appreciable economic perquisites of the office.

To turn to other phases of culture. In the Trobriands, myths are not owned quite so exclusively as in certain other areas, yet particular ones are associated with lineages who "are supposed to possess the most intimate knowledge of the mythical events, and to be an authority in interpreting them." Dances are more definitely individual property, the original inventor having the right to perform it in his village. "If another village takes a fancy to this song and dance, it has to purchase the right to perform it." Similarly, magical power—the knowledge of formulae intrinsically potent to achieve desired ends—is rated as a form of property. A very interesting analogy (though with a difference) to certain Plains Indian conditions may be noted. Sometimes matrilineal blood-relatives, who would be the natural heirs under aboriginal law, desire to secure certain goods in their elder's lifetime. In such cases substantial payments must be made by the nephew or the younger brother, e.g., the magic may be taught bit by bit in return for payment in instalments. "After the final payment, the title of ownership is definitely handed over to the younger man."¹⁶ The difference from the Hidatsa bundle concept lies in the fact that apparently material as well as incorporeal goods may be thus acquired by a Trobriander; and, further, an Hidatsa could not inherit the bundle except by making the customary payment.

Disabilities on sex lines introduce us to another category of incorporeal privileges. In the Banks Islands of Melanesia, women

¹⁶ *Ibid.*, pp. 185 ff., 291, 317, 329.

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never drink or prepare kava, and only in quite recent times have they been allowed to watch its preparation. "I never saw my informant more heartily amused," writes Rivers, "than when I told him that I had seen kava being made by a woman in Samoa."¹⁷ This evidently must be taken in conjunction with the widespread Melanesian and Australian custom of eliminating women from ceremonial life, for on the Banks Islands the use of kava was formerly restricted to those of high rank in the men's fraternity. The sexual division of labor, involving as it does an allotment of onerous tasks, seems to have little in common with the notion of property rights as commonly understood. But it may also imply greater or lesser kudos, and frequently very practical prerogatives. The Formosan women raise millet and sweet potatoes, and it is they, not their husbands and brothers, who superintend the granaries, dealing out daily supplies to the female representatives of the several households.¹⁸ On the other hand, in many African and Siberian tribes man's concern with the care of cattle disqualifies women from ownership, and cases are known in which remote kinsmen take precedence of daughters in the inheritance of livestock. In short, sex often includes the *right* to potential ownership of goods, corporeal or incorporeal.

This is naturally only a special form of group ownership. Wherever succession to dignity is regulated by some such principle as primogeniture, the entire group of incumbents, actual and potential, may be conceived as a corporation, the entire membership sharing the same privileges, even though possession and usufruct be limited to one individual at a time. It is this sense of a group interest that of course tends to nullify testamentary dispositions purporting to override established precedent.

A quaint coupling of prerogative with disability is reported from a people of the Upper Nile region. Though a Lango woman is never allowed to hold any but personal belongings, she may veto her husband's proposal to give away a single head of stock, provided it was obtained as a part of her daughter's bride-price.

¹⁷ W. H. R. Rivers, *The History of Melanesian Society* (Cambridge, 1915), p. 82.

¹⁸ J. B. M. McGovern, *Among the Head-Hunters of Formosa* (London, 1922), pp. 124 ff.

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The purpose of the custom is to safeguard the matrimonial prospects of a woman's son, for here, as elsewhere in Africa, the payment for a daughter is supposed to provide her brother with a wife. To put it differently, a woman who has given birth to a girl and married her off acquires thereby a limited control over the compensation offered.¹⁹

The Lango furnish us with another instructive sample of incorporeal property in disguise. Hunting-territories are owned individually, and without the owner's consent encroachment by other hunters would be illegal. However, there are responsibilities accompanying prerogative. The tract must be surrounded by a fire-break to ward off conflagrations spreading to or from the plots of fellow-tribesmen. What, it may be asked, is there incorporeal about land? The point is that no man really owns "his" *land*. Anyone desiring to build on it or to reserve a plot thereon for tillage cannot be denied, even though formal permission must first be sought. In other words, "the *won arum* owns the hunting rights over the land rather than the land itself." However, he is not liable for damage to the newcomer's house or crops.²⁰ This instance recalls comparable data from New Zealand, where the same territory was differently exploited by different households. One family would have a monopoly of the claim to the shellfish or berries found, another reserved the right to dig fern-roots or to hunt rats.²¹

Enough has been said to demonstrate the reality of incorporeal ownership on the level of illiterate peoples. How vital a part it plays in their lives is at once apparent when we recall the manifold ramifications of the subject inevitable even in this brief exposition. Starting from a juridical concept, we have had to touch the entire scope of cultural phenomena,—not only the proximate fields of social structure and government but the more remote departments of economics, industry, arts, and religion.

¹⁹ J. H. Driberg, *The Lango, a Nilotic Tribe of Uganda* (London, 1923), p. 172.

²⁰ *Ibid.*, pp. 112, 171.

²¹ Lowie, *Primitive Society*, p. 229.

Economic Factors and Culture

UTILITARIAN MOTIVES LOOM SO LARGE IN CONTEMPORARY life as to veil the force of other psychological drives. Thus an economic determinism has found favor in many circles far beyond the bounds of Marxian philosophy. As usual, sanity lies neither in spurning explanations that have appealed to serious scholars nor in clasping them fervently to our bosom as the long hoped for key to all mysteries, but in a discriminating appraisal of what is really solved and what eludes solution.

That man must have food in order to enjoy any social life whatsoever is an unchallenged truism. Yet even this can be invested with meaning by demonstrating the concrete way in which it affects particular situations. Among the lower hunters who are constantly obliged to shift their camps from the sheer need for food there are nevertheless periodic major festivals. How, on this level, is it possible to unite even a hundred people in one spot for a number of weeks or months? Evidently only if some lucky fluke creates a temporary surplus of food. In Tierra del Fuego, accordingly, it is the unpredictable stranding of a whale that precipitates a large assemblage and with it the pos-

This paper was found among Professor Lowie's unpublished MSS. Internal evidence suggests that it was written in 1939 or 1940.

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sibility of an initiation festival.¹ Again, among the Murngin of Northern Australia the great ceremonies never take place during the rainy season, nor at the beginning of the dry season when the grass is too high for travel and the food plants are not yet ready for harvesting. It is necessary to wait until the grass can be burned and the women can collect wild yams, lily bulbs, and cycad nuts as provisions for the ceremonial period.² Similarly, festive activities of the Shoshoneans of Nevada were restricted to occasions when "food supplies were sufficient to support an abnormally large number of persons for a week or so." This condition could be created by communal antelope hunts, rabbit drives, or pine-nut gathering.³

So far, so good. Ceremonialism would not be possible for our three tribes without the guarantee of ample food. But what of the ceremonial itself? Its alleged ends are partly economic: Australians may attempt to multiply game animals, Shoshoneans to increase the salmon run. But the ritualistic activity in its totality remains untouched by the economic theory. Why are women frightened away from the dance ground by the hum of bull-roarers? Why is the emphasis on the initiation of boys when so many American tribes celebrate only the girls' coming of age? The terrorizing of Australian women might be made to fit into the Marxian pattern as the exploitation of a downtrodden sex duped into furnishing provisions for the male celebrants; but why are they similarly cowed by the Ona of Tierra del Fuego, where practically all food is secured by men? Why do the Ona lavish infinite pains on the elaboration of masquerade costumes for their festival? These are but a few of myriad questions that arise: they and others like them form the core of the problems that arise for scholars concerned with the study of ceremonialism. They are not answered by the axiom that the phenomena would be precluded if the would-be-performers were dying of starvation. Nor is it any help to be told that the

¹ Martin Gusinde, *Die Yamana; vom Leben und Denken der Wassernomaden am Kap Hoorn* (Mödling bei Wien, 1937), pp. 823 f.

² William Lloyd Warner, *A Black Civilization* (New York and London, 1937), pp. 340, 347.

³ J. H. Steward, *Basin-Plateau Aboriginal Sociopolitical Groups*, BAE, Bull. 120 (1938), pp. 45 f., 237.

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celebrants are seeking satisfaction of vital needs. No one denies that their activities are motivated: the problem is whether the needs and motives are preponderantly, nay, exclusively, bound up with the food-quest; and this is clearly contrary to all experience.

One main objection to the economic interpretations of culture that have hitherto been offered is that they fail to come to grips with those problems that obtrude themselves spontaneously on any unprejudiced observer of the data. The theorists of this school ignore even so obvious a fact as the irrational ingredients of economic activity, a fact so convincingly established by the late Eduard Hahn and so overwhelmingly corroborated by later inquiry. To take a single example, East Africans are enthusiastic stock-breeders; but is their animal husbandry to be gauged by our standards? Far from it. A Shilluk keeps hundreds of cattle, yet slaughters them so rarely that he is obliged to maintain his hunting techniques for an adequate supply of meat. His small cows yield but little milk, his oxen normally serve no purpose at all. But these Negroes, who have failed to perfect their dairying industry and who eschew a beef diet, expend enormous effort on massaging the humps of their beasts and twisting their horns into grotesque shapes.⁴

In other words, man spontaneously exerts himself not merely in order to fill his belly, but also for utterly fanciful objectives. If the domain of economic life itself is shot through with non-economic motivation, it is a fortiori hopeless to derive art, literature, philosophy, and religion from an exclusive interest in food and profit.

Nothing is more certain to prejudice a sound economic interpretation than such unsupportable claims on the part of enthusiasts like Dr. Paul Radin, whose merits as a field investigator must not blind us to theoretical biases.⁵ The remarkable thing about this attempt to account for aboriginal beliefs is its persistent *neglect* of economic facts while rendering on every other page lip service to economic determinism. The picture of the Central

⁴ Wilhelm Hofmayr, *Die Schilluk* (Mödling bei Wien, 1925), *passim*.

⁵ Paul Radin, *Primitive Religion; Its Nature and Origin* (New York, 1937), esp. pp. 51 ff.

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Eskimo is typically Voltairean: His shamans, organized to cow the laity, terrorize the masses by an elaborate mechanism of beliefs, formulae and taboos. They have deliberately devised the system to keep all contact with the supernatural in their own hands and to exploit the superstitious fears of their dupes. Thus, the shaman may abduct his neighbors' wives and tyrannize them generally with impunity since no one dare rebel against, let alone murder, a communicant with the spirit world; accordingly, he conquers economic insecurity and "life flows on for him in comparative ease."

Not a single one of these propositions bears even a remote resemblance to the facts. The Eskimo shamans are not organized; they may bully their communities, but a medicine-man suspected of kidnapping souls is killed; and ordinary people possess talismans and magical formulae, so that supernatural power is by no means confined to the shamans. An old Iglulik woman who had inherited a famous magical spell was able to achieve "economic security" by selling it to a hunter, who pledged himself to provide her with food and clothing as long as she lived. In other words, she got herself an annuity.⁶

The basic error, however, lies deeper. Dr. Radin wholly disregards the vital fact that the insecurity of which he speaks does not exist in a social sense among the Eskimo: so long as food is available *no* one suffers from want of the bare necessities. Even the idler gets his meals; he simply sinks in public esteem. That is the typical primitive attitude, as amply attested for Melanesia.⁷

In short, the "wealth" of an Eskimo shaman or a Melanesian magician does not make him more secure as to the bare necessities of existence; it simply confers prestige. But prestige is an *ideological* phenomenon.

The correct approach to an economic interpretation is not to deal in vague generalities, but to isolate *specific* elements in the productive and distributional system of peoples and to correlate

⁶ Franz Boas, *The Eskimo of Baffin Land and Hudson Bay*, AMNH Bull., XV (1907), 117 f.; Knud Rasmussen, *Intellectual Culture of the Iglulik Eskimo* (Copenhagen, 1929), pp. 144, 149, 165.

⁷ Rasmussen, *op. cit.*, p. 159; Kaj Birket-Smith, *The Caribou Eskimos* (Copenhagen, 1929), p. 261; Hortense Powdermaker, *Life in Lesu* (New York, 1933), p. 224.

these with effects that can be reasonably traced to them. In this inquiry a much finer analysis is required than is customary. Thus, the influence of "agriculture" is not at all what it is commonly supposed to be. In its incipient phases "agriculture" produces no spectacular results either in density of population, or stability, or cultural superstructure. Because aboriginal farmers cannot work heavy soils with crude dibbles, they avoid rich lands in favor of inferior ones. In consequence, less than one per cent of the possible area within the Eastern United States was cultivated by the indigenous redskins; and their density of population fell below that of purely hunting Californians.⁸

That "farming" in itself harbors no magical potency is likewise indicated by Lapp experience. The sedentary Lapps of Kautokeino are decidedly below par as compared with their nomadic brethren. One of these farmers raises not cereals for human consumption, but fodder; his livestock comprises typically a few sheep, a horse, from two to a dozen cows; he produces little or no butter and cheese. Without herds of reindeer, he must nevertheless keep a few head of these animals for winter transport where horses are ill adapted to conditions of travel. In the summer some nomad friend will allow them to pasture with his own herd. Thus, there is a certain dependence of the "agriculturists" on the pastoralists; and, even so, existence is possible only because the farmer catches fish in the summer.⁹

Similarly, we cannot lightly assume that the economically productive sex or class in a community must be the dominant one. Much has been written about the ascendancy of women because of their putative invention of farming, but little support is to be found for this view. In Uganda a woman with a good plantain grove feeds three or four men, but there is no question of feminine equality.¹⁰ On the other hand, the Hopi Indian men grow maize, but the women, though in no sense matriarchs, do own the

⁸ Carl O. Sauer, "American Agricultural Origins: A Consideration of Nature and Culture," *Essays in Anthropology Presented to A. L. Kroeber* (Berkeley, 1936), pp. 279-298; A. L. Kroeber, *Cultural and Natural Areas of Native North America*, Univ. Calif. Publ. Amer. Arch. and Ethn., XXXVIII (1939), 146-150.

⁹ P. L. Smith, *Kautokeino og Kautokeino-Lappene* (Oslo, 1938), pp. 300-318, 566-572.

¹⁰ John Roscoe, *The Baganda* (London, 1911), p. 431.

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fields and houses. Evidently there is no *simple* relation between status and productiveness.

A much more fruitful attack is to envisage the total economic situation, isolate one factor, and note its correlates. The Ona of Tierra del Fuego are guanaco hunters, whose wild vegetable fare is negligible, so that women contribute nothing essential to the larder. Under the hard conditions of Fuegian life, then, polygyny can be only most exceptional,—in contrast to its efflorescence where women largely add to the food supply, as in Africa. Even in warfare an Ona will generally liberate a captive woman since she merely represents an extra mouth to feed.¹¹

Among the Chukchi of northeastern Siberia it is also possible to make a worth-while analysis. These people were originally all maritime hunters of cetaceans, but in recent centuries a large part of the population turned to reindeer-breeding. What, we may ask, has been the effect of this new economic element on the position of woman? The results have been definite, but are not easily pigeonholed in such rubrics as “superior” and “inferior” status, for in both groups man is dominant. However, there is a significant difference: the Maritime Chukchi naturally restrict the perilous chase of sea-mammals to men; among the pastoralists women help tend the herds, hence work harder, but with the advantage of being sometimes in the position of owning a herd. Further, celibacy is rarer, polygyny more frequent, among the nomads. For while a Maritime Chukchi may find it difficult to support a single wife, a Reindeer Chukchi requires a wife for each of his herds.¹²

Equally suggestive are the consequences of equestrianism among post-Columbian Redskins. The earlier Shoshoneans of southern Idaho, like their western congeners, lived in an arid country poor in big game, so that salmon and wild roots were their staple diet. But the horse introduced by whites revolutionized aboriginal economy. It facilitated the killing of large beasts, as well as travel across the mountains in search of bison. Because of the resulting profusion of hides the skin-dressing industry

¹¹ Martin Gusinde, *Die Selk'nam* (Mödling bei Wien, 1931), *passim*.

¹² Waldemar Bogoras, *The Chukchee*, AMNH, Memoirs, XI (Leiden, 1909), *passim*.

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prospered, and rawhide or other skin containers partly replaced basketry. Hitherto these tribes had been riveted to the vicinity of stored supplies; now they acquired greater mobility. Thus, too, large aggregations of people became possible: it was no longer essential to split up into minute family groups for most of the year in order to gain subsistence; permanent bands evolved as larger politico-social units. The innovation was also potent indirectly. Horses lured thieving raiders from the Plains and precipitated warfare. This not only fortified band coherence, but led to the adoption of sundry Plains features,—skin tents, shields, war honors, even new ceremonies.¹³ Thus, the introduction of a single transport animal had far-reaching ramifications that could not be easily foreseen.

It is clear, then, that economic factors may be real and potent. But, apart from the truism that man must eat in order to live, they do not explain the whole or even a preponderant part of culture. The point for the scientific investigator is to determine precisely how far their influence extends; and that is best done by examining the largest possible number of instances in which an historically authenticated economic element has been injected into a culture.]

¹³ Steward, *op. cit.*, pp. 46, 201 ff.; *idem*, "Changes in Shoshonean Indian Culture," *Scientific Monthly*, XLIX (1939), 524-537.

Property Rights and Coercive Powers of Plains Indian Military Societies

AMONG THE FOUNDERS OF COMPARATIVE SOCIOLOGY HEINRICH Schurtz deserves a lasting place of honor. Prior to his *Altersklassen und Männerbünde* (Berlin, 1902) the wide distribution and social significance of associations in primitive life had escaped the notice of theorists, who confined themselves largely to problems connected with marriage, the family, and the clan. Schurtz's pioneer effort was far from flawless. There are facile generalizations on the psychology of sex and irrelevant personal opinions that may be charged to the exhibitionism of youth. The author also misinterprets ethnographic evidence from sheer ignorance of the facts, which at that time were sadly wanting for many areas. Yet in historical perspective the book commands respect as one of the landmarks in the development of the science.¹

Journal of Legal and Political Sociology, I (April 5, 1943), 59-71. In somewhat different form the second part of this paper was read at one of the Fiftieth Anniversary symposia of the University of Chicago, September, 1941, but has hitherto remained unprinted.

¹ For a critique, see R. H. Lowie, *Primitive Society* (New York, 1920), chap. xi.

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One of Schurtz's cardinal postulates was the function of age differences as creators of social units: age-grades, he contended, were humanity's earliest *deliberate* attempt at social segmentation. Thus he naturally came to impress into service the remarkable information Prince Maximilian of Wied-Neuwied had collected among the Mandan and their neighbors of the upper Missouri in the early thirties of the last century.² For the Prince reported from each tribe a series of men's societies differentiated by emblems, songs, dances, behavior, and age. Growing older was not the sole basis of a higher membership, to be sure, since payments were prerequisite. But any thinker who assumed a normal sequence of stages in social development could easily dispose of the complication: Schurtz simply explained the Plains Indian phenomena as transitional from the earlier type of pure age-classes to the as yet unattained type of the pure club in which membership rests solely on payment of a fee.³

Field research subsequent to Schurtz and largely stimulated by his theories brought out a fact he could not have gleaned from Maximilian. Though specific societies were often closely paralleled in alien tribes, the pattern found by the Prince as underlying the entire tribal system of military organizations turned out to hold for only a bare half dozen Plains groups. Everywhere else societies were neither graded by age nor entered by payments. This in itself was no refutation of Schurtz's chronology: conceivably the rarer pattern might nevertheless be the older. However, the reverse assumption became a priori equally legitimate.

More vital was a discovery in part already to be gleaned from the earlier sources. A particular organization, such as the widespread Kit-fox society, was not indissolubly linked with a particular age: a young men's company on the upper Missouri, it ranked much higher in the Blackfoot (Montana, Alberta) scheme, and was of course coördinate with other organizations in the ungraded systems. Even within a single tribe the place of a

² For a summary of these and later data see Clark Wissler, R. H. Lowie, *et al.*, *Societies of the Plains Indians*, AMNH, Anthropological Papers, Vol. XI.

³ Heinrich Schurtz, *Altersklassen und Männerbünde* (Berlin, 1902), pp. 83 ff., 151-165.

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military society varied with the lapse of time. Whereas Maximilian's Blackfoot in 1833 rated the Dogs as inferior to the Ravens, all subsequent observers reverse the order. Similarly, Curtis' Braves in the same tribe comprised the oldest bachelors, but according to Wissler they were predominantly young married men.⁴ Membership in association A, B, C, and so on, was therefore devoid of an intrinsic, immutable age status. Its basic meaning must be sought elsewhere.

The clue came through the disruptive agency of modern conditions. Because of the ensuing upheaval, companies of men who would normally have jointly acquired a higher society failed to buy a new membership. It turned out that these men did not automatically rise to a higher grade, but remained in the societies they had bought. However, the native attitude went much further: in 1910 a Hidatsa (North Dakota) about 90 years old considered himself *simultaneously* a member of organizations joined at about seven, twenty, twenty-seven, and forty-five, respectively. Identical conceptions obtained among the Mandan (North Dakota) and Blackfoot.⁵ The variations among the Arapaho (Wyoming) and Gros Ventre (Montana, Alberta) are negligible in this context, for they harmonize with the principle that membership in a graded society of the Plains was a form of negotiable property the claim upon which never lapsed until it was sold. The very tribes, then, which apparently stressed age-stratification in their graded series in actuality considered *purchase* as the basic principle of affiliation.

Whence, however, the empirically established fact that at any one time fellow-members were virtually all contemporaries? Here Schurtz stands vindicated. He erred, indeed, in asserting the necessary priority of associations uniting the age-mates of a community, but he was right in ascribing to coevals the solidarity that could create social units and thereby differentiate them from one another. There is abundant evidence from our area that young boys were wont to foregather as a spontaneous,

⁴ Clark Wissler, *Societies and Dance Associations of the Blackfoot Indians*, AMNH, Anthropological Papers, XI (1913), 365-381. Among people who do not reckon their ages exactly by years, matrimonial status is significant in this context, as Schurtz recognized.

⁵ *Ibid.*, pp. 427, 972-975.

informal age-class in imitation of their elders. In tribes with a system of crystallized age-societies nothing would be more natural than for these boys collectively to buy the right to the lowest organized society, and later to continue buying successive memberships at suitable intervals. We must distinguish, then, two senses of the term "society." On the one hand, we are dealing with sets of incorporeal property rights. Any one complex was not intrinsically linked with a particular age grade, so that shifts in point of age occurred either through internal rearrangements or when a society was borrowed (or bought) by an alien group. On the other hand, the gang of united youngsters would regularly remain together and subsequently buy their way up to the top of the scale. These age-mates, then, do form a body independent of their joint property rights; for during the interval between their sale of a particular membership and their purchase of another their sense of solidarity would persist unabated even though joint activity might dwindle to a minimum.

This distinction appears most clearly among the Gros Ventre (Montana, Alberta), where each gang bore a unique name but shared a set of "dance" privileges with several other gangs, i.e., with adjoining age-groups which performed like dances with like regalia, but always independently of one another. Thus, a man for a lifetime labeled as a Holding-to-a-dog's-tail was reckoned a Dog only during and after the performance of the Dog ceremony, sharing *that* appellation with the other gangs owning the same prerogatives.⁶

As to the property rights of the age-societies in general, several points are worth noting. As just explained, several groups might equally share in the "copyright" to a particular complex. To a more limited extent other tribes than the Gros Ventre permitted the sharing of the same emblems or other integral parts of complexes by distinct bodies. This fact might also be phrased by saying that the same item could enter distinct complexes. Furthermore, both the acquisition of membership and the rights obtained were only in part collective. A Hidatsa gang jointly made a large soliciting gift to a higher group in order to coax them into

⁶ *Ibid.*, p. 933. A. L. Kroeber, *Ethnology of the Gros Ventre*, AMNH, Anthropological Papers, I (1908), 232.

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selling their society; but subsequently each buyer might have an individual seller as his ceremonial "father," obtaining by the transaction distinctive privileges such as go with honorific offices. Finally, there are the tribes with ungraded systems and without purchase. Here, e.g., among the Crow (Montana), one gang did not replace another, but individuals spontaneously joined or were invited to add to the numerical strength of a society. Undeniably the members here also had property rights: these could be lost, as when one of two rival Crow organizations excelled the other in martial exploits and thereby won the right to monopolize the losers' tunes for one season. However, from the legal point of view, it is an important matter whether possessions are, or are not, negotiable: Inalienable land is property in a different sense from land that can be disposed of at will.

The curious point in this connection is that the tribes in question are by no means unfamiliar with the concept of selling incorporeal property, such transactions being in fact a constantly recurring phenomenon among the Crow (Montana), who used to pay a horse even for such minor privileges as painting the cheeks with a certain design. Why, then, did they fail to extend so deep-rooted a conception to their military societies? Without generalizing for the entire area I should suggest the following explanation for the Crow. These Indians did sell a variety of privileges, but these come exclusively, or at least preponderantly, into the category of *sacred* possessions, i.e., they were directly or indirectly traced to supernatural revelations. But their military societies were devoid of religious meaning; a few extremely faint reminiscences of visionary experiences to explain the origin of certain organizations could not arrest their thorough-going secularization. The complex of badges, songs, and activities of a military society was thus valued quite differently from the corresponding complex of the sacred Tobacco societies and fell into a distinct category in Crow consciousness.

II

For a large portion of our area and adjoining regions to the east and north early sources attest a minimum of governmental authority. Denig's description of an Assiniboiné council, Tanner's

account of a joint enterprise by Ojibwa (Great Lakes), Cree (Central and Western Canada), and Assiniboiné (Western Canada, Montana), Tabeau's observations among the Dakota and Arikara (North Dakota), Hearne's experiences with his Chipewyan (Athabaska-Mackenzie area) guide Matonabbee are all mutually corroboratory on this point. According to Jones's summary of aboriginal Ojibwa conditions, e.g., there were councilors with narrowly localized jurisdiction to which status every tribesman was admissible. Their powers were vague and limited, and the chief chosen by them was even less able to alter existing custom at his pleasure. In Denig's day the head chief of the Assiniboiné was a purely nominal leader who lacked special prerogatives and could be humiliatingly overruled by the council. The chiefs known to Tabeau were unable to quell a riot and might have their authority set at naught by a single resolute individual: "insubordination and discord" reigned supreme.⁷

Making due allowance for exaggerations we find concordant testimony, perhaps not for the anarchy implied in some of the statements if taken literally, but at least for a marked freedom of the individual from physical restraint. This impression is strengthened by the widely held definition of ideal public functionaries. The Pawnee (Nebraska) chief, far from being a sovereign ruler, was above all a peace-maker and guardian of the village, his Hidatsa colleague was "a man of general benevolence who offered smoke to the old people and feasted the poor." Their counterpart among the Plains Cree was not only expected to exercise generosity, but to sacrifice his property for the maintenance of order, nay, to forgo vengeance if one of his own kinsmen was slain. Correspondingly, a Winnebago (Wisconsin) chief constantly distributed his possessions and interceded between evil-

⁷ E. T. Denig, "Indian Tribes of the Upper Missouri," BAE, 46th Ann. Rept. (1930), pp. 431 ff., 435-440, 449; Edwin James, ed., *An Indian Captivity (1789-1822): John Tanner's Narrative of His Captivity among the Ottawa and Ojibwa Indians, 1830*, Sutro Branch, California, State Library, Reprint Series, No. 20, chap. xi, p. 151; Annie Heloise Abel, ed., *Tabeau's Narrative of Loisel's Expedition to the Upper Missouri* (Norman, Okla., 1939), pp. 104-106, 126; William Jones, "Central Algonkin," in *Annual Archeological Report for 1905* (Toronto, 1906), p. 137; Samuel Hearne, *A Journey from Prince of Wales' Fort in Hudson's Bay to the Northern Ocean* (London, 1795), *passim*.

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doers and their revengeful victims; he went so far as to mortify his own flesh in order to arouse the pity of the aggrieved, thus deflecting their anger from the culprits. In these tribes, then, the chief was essentially an appeaser working by cajolery. Small wonder that his adjutants and what has been called the camp police operated under like restrictions. The duty of all Plains Cree of superior status was to prevent strife; and the Black Mouths of an Hidatsa village removed misunderstandings, conciliating aggrieved tribesmen by gentle words and compensatory gifts.⁸

In view of these facts it is startling to find the liberties of the Plains Indian periodically suspended by something very much like martial law enforced by a body vested with supreme power for the time being. Probably the earliest report is Hennepin's. In 1680 the explorer met a Santee Dakota (Western Woodlands) party, who freely shared their recently obtained supply of buffalo meat. Suddenly:

Fifteen or sixteen Savages came into the middle of the Place where we were, with their great Clubs in their Hands. The first thing they did was to over-set the Cabin of those that had invited us. Then they took away all their Victuals, and what Bears-Oil they could find. . . .

We knew not what these Savages were at first. . . . One of them . . . told me, that those who had given us Victuals, had done basely to go and forestal the others in the Chase; and that according to the Laws and Customs of their Country, 'twas lawful for them to plunder them, since they had been the cause that the Bulls were all run away, before the Nation could get together, which was a great injury to the Publick; For when they are all met, they make a great Slaughter amongst the Bulls; for they surround them so on every side that 'tis impossible for them to escape.⁹

Tabeau, who was particularly familiar with the Arikara and Teton Dakota, tells us that the "soldiers" when elected "to watch

⁸ George A. Dorsey and J. R. Murie, *Notes on Skidi Pawnee Society*, Field Museum of Natural History, Anthropological Series, XXVII (1940), 113; R. H. Lowie, *Notes on the Social Organization and Customs of the Mandan, Hidatsa, and Crow Indians*, AMNH, Anthropological Papers, XXI (1917), 18 f.; David G. Mandelbaum, *The Plains Cree, ibid.*, XXXVII (1940), 221, 230, 231 f.; Paul Radin, "The Winnebago Tribe," BAE, 37th Ann. Rept. (1923), pp. 209 f., 227.

⁹ Louis Hennepin, *A New Discovery of a Vast Country in America* . . . (London, 1698), pp. 187 f.

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over the carrying out of the laws of the *cerne* [buffalo surround] or over that of some public decision," have the right—only temporarily, he emphasizes—"to be severe arbitrarily towards every delinquent, to kill his dogs, his horses, to break his weapons, to tear the lodges into tatters, and to seize . . . upon all that which belongs to him."¹⁰ This corresponds well to the composite picture from a variety of sources on different groups. One would merely like to add that though a refractory culprit might be severely beaten and even killed, a penitent was rewarded with a new lodge and more goods than had been destroyed. Tabeau admits other public functions, but is particularly impressed with the direction of the collective hunt (*c'est surtout le cas ou les soldats sont sévères dans l'exécution de leur charge*) and justifies the law as absolutely necessary because to transgress would be to "*détruire ainsi la base de la subsistance générale (sic)*."

It was observations of this type that I once combined with principles enunciated by Schurtz into an hypothesis for the evolution of the State. My problem was to account for the rise of territorial sovereignty from a condition in which, as earlier theorists had averred, kinship provided the only bond for joint political action. Schurtz himself had already argued that a closely knit primitive secret organization could inject order into communal life such as transcended the power of weak chiefs or was precluded by the constant bickering of rival clans.¹¹ Among the best-known Plains peoples the temporary coercive authority was generally vested in military organizations, one or more of which units were empowered to punish the crime of stampeding buffalo. Thus, these Plains associations seemed a potential instrumentality for territorial integration, achieving intermittently, i.e., during the surround, what a modern State professes to do continuously. Subsequently the hostilities among the several associations within a tribe made a strong impression upon me and their mere existence appeared as potentially no less disruptive than that of contending blood-groups. Nevertheless, I retained the idea that

¹⁰ Abel, *op. cit.*, pp. 116 ff., 245.

¹¹ Heinrich Schurtz, *op. cit.*, p. 363 *et passim*; R. H. Lowie, *Primitive Society*; *idem*, *The Origin of the State* (New York, 1927).

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the hunt-directing police force illustrates "the meteoric display of sovereign authority" in "an almost anarchic community."¹²

Naturally the question obtrudes itself why an approach to complete freedom should thus alternate with subjection to coercive force. For this there readily suggested itself an answer in economic terms. Securing an ample supply of buffalo meat was a matter of life and death, hence under the threat of starvation the Indians willingly surrendered their normal rights, subjecting themselves to a rigorous discipline.

In recent years Drs. Hoebel and Provinse have independently criticized my position.¹³ They have not, however, rejected my specimens of compulsion as spurious, nor do they consider them irrelevant to the problem of political development. The gist of their comments is rather that I have understated the case: "Sovereignty"—to use a grandiloquent term which I invest with no fetichistic reverence in this context—is, they argue, less sporadic and more inclusive than my exposition suggests. It was misleading to overemphasize the spectacular disciplinary concomitants of the buffalo hunt. My own field data, as well as those of other observers, are aptly cited to show that the same procedures held in several distinct circumstances.

I accept the criticism as valid and should like to strengthen it by additional evidence. At the same time I must qualify some of its implications.

Ethnographic facts that contravene the unique significance of the buffalo surround come from the area to the east of the Plains. As Skinner and Macleod have noted,¹⁴ several Woodland peoples, i.e., peoples for whose economy buffalo-hunting was a

¹² Lowie, *The Origin of the State*, pp. 107 ff., 116.

¹³ E. Adamson Hoebel, "Associations and the State in the Plains," *American Anthropologist*, XXXVII (1936), 433-438; *idem*, *The Political Organization and Law-Ways of the Comanche Indians*, AAA, Memoirs, No. 54 (1940), p. 82; John H. Provinse, "The Underlying Sanctions of Plains Indian Culture," in Fred Eggan, ed., *Social Anthropology of North American Tribes* (Chicago, 1937), p. 365.

¹⁴ Alanson Skinner, *Social Life and Ceremonial Bundles of the Menomini Indians*, AMNH, Anthropological Papers, XIII (1913), 22-26; *idem*, *Material Culture of the Menomini* (New York, 1921), pp. 51 f.; *idem*, *Political Organization, Cults, and Ceremonies of the Plains—Ojibway and Plains Cree Indians*, AMNH, Anthropological Papers, XI (1914), 498 f.; Radin, *op. cit.*, pp. 114, 209, 220, 226 f.

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subordinate or even negligible feature, paralleled the phenomena so strikingly manifested by their western neighbors. The Winnebago constabulary did control the chase, but—like the Menomoni (Wisconsin) counterpart—they also proceeded in similar fashion to forestall premature exploitation of wild rice. The Sauk and Fox (Wisconsin) “war chiefs” directed not the hunt itself, but the homeward journey from it, their aim being to preclude hostile attacks on single families and the pillaging of corn by nimble marauders. Every night one war chief would set up his staff as a boundary mark, and whoever stepped beyond it had “his canoe and whatever else he may have along with him destroyed.” As soon as the village routine was restored this martial law lapsed; in no other circumstance did our authority discover “any laws enforced or penalties exacted for disobedience of them.” The Winnebago police had a variety of functions beyond those already mentioned. Besides preventing a stampede of game and premature inroads on wild rice, they regulated travel in the Sauk and Fox fashion, guarded the village continuously, and whipped seducers of women. Oddly enough, in cases of murder these coercers figured as appeasers.

It seems idle to speculate whether the police institution as we find it in the Plains originated there or in the Woodlands, especially since we know that many of the “Plains” tribes emigrated to their historic homes from the forested regions in relatively recent times. But it is evident that coercive functions are not indissolubly tied up with the conditions of hunting herds of big game. As indicated they coexist with radically distinct pursuits. Further, they do not automatically arise from the buffalo surround. For the Sarsi (Alberta), the Comanche (Texas), the Shoshone (Idaho, Wyoming), marginal but in many ways thoroughly acclimatized representatives of the Plains, hence devoted buffalo hunters, either lacked punitive measures or reduced them to a minimum.¹⁵

Hoebel’s and Provinse’s criticisms are therefore well taken; the

¹⁵ Diamond Jenness, *The Sarcee Indians of Alberta*, National Museum of Canada, Bull. 90, Anthropological Series, No. 23 (1938), pp. 11, 41; Dimitri Shimkin, personal communication concerning the Wind River Shoshone; Hoebel, *The Political Organization . . . of the Comanche Indians*, p. 82.

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range of coercive powers did not coincide with buffalo hunting but embraced various other aspects of tribal life in the Plains and adjoining regions. As already indicated, this revision involves at bottom not a refutation but a strengthening of my basic contention, for it asserts a far broader base than the narrow economic one envisaged by me.

Though this is very gratifying, I must turn devil's advocate against the possible implication that the police phenomena demonstrate more than incipient Statehood. On the one hand, as explained above, the genuinely authoritarian aspects of various tribal constabularies (Crow, Winnebago) are superseded by purely persuasive functions in so vital a crisis as intratribal murder.

Secondly, we must recognize the seasonal dichotomy of social life among many Plains tribes, roughly paralleling the contrasts emphasized by Durkheim for the Australians and by Mauss for the Eskimo.¹⁶

Wissler stated the facts long ago,¹⁷ pointing out the intermittent character of governmental control in our area, but my previous publications fail to give due emphasis to the data. A few representative phenomena may be cited. The Blackfoot held their council in the summer, separated after the fall hunt, and re-assembled in the spring. Hidatsa villages were under a winter chief whose term began in autumn and ended with the melting snow. The Dakota and the Crow reorganized their military societies every spring and these companies functioned until the first snowfall. Three recent monographs bring out the essential point with startling clarity. For the Cree, the Sarsi, and the Kiowa (Oklahoma) social life culminated in the annual Sun Dance. It was for that ceremony that Cree bands would unite in the summer, soon thereafter dispersing and finally, in mid-winter, breaking up into minute family groups too small to permit associational activity. Similarly, the Sarsi hunted buffalo in small tribal segments, and only for the Sun Dance one society, the Red Paint organization, assumed control. Again the Kiowa

¹⁶ Marcel Mauss, "Essai sur les variations saisonnières des sociétés Eskimo: Étude de morphologie sociale," *L'Année sociologique*, IX (1906), 39-132; Emile Durkheim, *Les Formes élémentaires de la vie religieuse* (Paris, 1912), *passim*.

¹⁷ Clark Wissler, *The American Indian* (New York, 1922), pp. 161, 178.

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associations "functioned only during the four or so weeks of the Sundance gathering."¹⁸

The lapse of authoritarianism for a considerable, or in some tribes even the major, part of the year as a result of economic conditions and consequent modes of settlement, inevitably reduces whatever sovereignty exists to a nascent stage.

Finally, as Provinse aptly shows, there may be dispersal of authority between two or even more agencies. For example, normally during the season of reunion the Crow camp would be under the guidance of a chief and the military society appointed by him; but at a Sun Dance the director of that ceremony is supreme and appoints another association to serve for its duration.¹⁹ Such temporal alternation of supreme power evidently militates against the centralization of authority.

However suggestive, then, the data on police functions in and near our area are, they suggest nascent rather than achieved governmental integration. The failure to extend the coercive authority of police associations over the entire field of internal relations (notably in the case of murder); the merely periodic, in some cases even ephemeral, assumption of such authority; its dispersal between two or more foci; the disruptive tendency of strife between rival associations pointed out in *The Origin of the State*—all militate against the creation of a full-fledged State.

Nevertheless, the potential jurisdiction of a military society remains a most significant fact for our theme. This has been very effectively demonstrated for the Cheyenne (Montana). Although here, too, the seasonal dichotomy breaks up the societies during the fall and winter, this did not apply to the Dogs, who by an historical accident coincided with one of the tribal bands, hence preserved a unique solidarity. In fact, some semblance of cohesion obtained even in the other societies inasmuch as members who happened to reside together during the period

¹⁸ *Idem*, *The Social Life of the Blackfoot Indians*, AMNH, Anthropological Papers, VII (1911), 22–26; *idem*, *Societies and Ceremonial Associations in the Oglala Division of the Teton-Dakota*, AMNH, Anthropological Papers, XI (1912), 24; Lowie, *Notes on the . . . Mandan, Hidatsa, and Crow Indians*, pp. 18 f.; Mandelbaum, *op. cit.*, pp. 203, 225; Jenness, *op. cit.*, pp. 11, 41 f.; Jane Richardson, *Law and Status among the Kiowa Indians* (New York, 1940), pp. 9 f.

¹⁹ R. H. Lowie, *The Crow Indians* (New York, 1935), p. 308.

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of dispersal might join in action as occasions arose. Possibly most illuminating is a case of mayhem. Among Plains Indian peoples generally such an offense was only a tort, the police assuming the function of mere go-betweens in trying to persuade the aggrieved party to accept an idemnity. Here, however, something radically different occurred: the Foxes then serving as police treated the matter as a public wrong, severely beat the culprit, and accepted the "fine" offered by him without sharing it with his victim.²⁰ This certainly suggests that the governmental powers of a military organization were capable of very considerable enlargement.

One question which remains is whether the germs of sovereignty were peculiarly tied up with the rise of military associations. Reverting to the ethnographic correction made above concerning the supposedly unique effect of the buffalo surround, I should like to point out that the geographical extension of our police phenomenon automatically supplies an answer. For in the Woodlands there are no military societies of either the graded or ungraded type. Moreover, it has long been recognized that the conventional "Plains Indians" comprise a congeries of culturally disparate groups. Some are matrilineal, others patrilineal, still others without definite rules of descent. In one sector of the area kinship nomenclature is of the Omaha type, almost exactly duplicated in the western Woodlands; in another we find its precise logical antithesis, whose closest parallels crop up in the Southeast and in Arizona; still another province displays a third pattern. Agriculture is totally unknown to some tribes, rudimentary among others, more highly developed among the Pawnee. The Sun Dance, the dominant festival of the high Plains, dwindles to insignificance or disappearance among Southern Siouans, who in part substitute an equivalent of the Woodland ceremonial dramatizing the ritual killing and revival of initiates. These facts long ago recognized in conversation by the late Alanson Skinner have at last been adumbrated taxonomically by the suggestion of a "Wisconsin-Prairie" area.²¹

²⁰ K. N. Llewellyn and E. A. Hoebel, *The Cheyenne Way* (Norman, Okla., 1941), chap. v, esp. pp. 99-101, 110 f., 115, 118 f., 122 ff.

²¹ A. L. Kroeber, *Cultural and Natural Areas of Native North America*, Univ. Calif. Publ. Amer. Arch. and Ethn., XXXVIII (1939), 85.

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Now within the Prairie subdivision of this area we discover a feature shared not only with the complementary Wisconsin sector, but with a variety of still more easterly Woodland tribes: public functions of one sort or another devolve on the *clan*. Thus, it is the Bear clan that policed a Winnebago village, where, incidentally, the chief as an appeaser belonged to another, the Thunderbird, clan. This does not necessarily mean that all adult men of a certain clan formed the permanent police or that all the members of a squad were clansmen; the point is rather that at least the responsibility for recruiting and supervising the police rested upon one or more specific clans. Thus, the two Osage (Missouri) chiefs, each representing one of the two moieties and a particular "gens," i.e., patrilineal clan, within it, appointed the marshals for the hunt, one from each of certain gentes; and honorific titles devolved on three of the officers, each representing one of these units. Similarly, among the Iowa "the Elk gens furnished the soldiers or policemen"; and according to Fletcher and La Flesche, two clans were associated with the regulation of the hunt among the Ponca²² (Nebraska).

The recruiting of a constabulary from definite clans is not, however, the only alternative to identifying it with a particular military association. Several Plains tribes used the device of what may be called a "nonce police." The Pawnee, for example, though vesting disciplinary powers within the village in the hands of the chief's adjutant and three appointees of his, regulated the buffalo hunt on a different principle, a priest selecting one of four possible organizations *for that particular enterprise only*. So the Omaha council would delegate to men from the class of brave warriors the task of controlling some communal hunt. The appointees formed no permanent body, but owing to their fitness were likely to be subsequently drafted for police duties in the village. From sources on the Plains Ojibwa, Dakota, and Assiniboine it seems probable that wherever men of recognized valor formed a distinct class they were the obvious

²² Francis La Flesche, "The Osage Tribe," BAE, 36th Ann. Rept. (1921), pp. 66-68; J. O. Dorsey, "Siouan Sociology," BAE, 15th Ann. Rept. (1897), p. 238; Alice C. Fletcher and Francis La Flesche, *The Omaha Tribe*, BAE, 27th Ann. Rept. (1911), pp. 45, 210, 279.

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candidates for constabulary duty. In short, *any* preëxisting unit clearly defined in native consciousness, whether military club, clan, or distinguished warrior group, readily lent itself to such assignment.²³

Summarizing the above remarks, I still feel that the military organizations of the Plains area exemplify the potentialities of associations as regards the creation of supreme central authority. It is merely necessary to remember that such germs of Statehood actually as a rule remained rudimentary, though the Cheyenne case demonstrates that some tribes carried them much nearer to fruition than others. Further this type of unit is obviously not peculiarly fit to absorb disciplinary functions. Where military societies are lacking, such activities quite as naturally devolve on other preëxisting units, such as clans or a general honorary class of braves.

Note.—For the orientation of the reader I list alphabetically the location of Indian tribes mentioned in the article. Where the habitat changed repeatedly in historic times, one or two significant locations are taken to suffice for present purposes.

Arapaho—Colorado, Wyoming	Hidatsa—N. Dakota
Arikara—N. Dakota	Iowa—Iowa
Assiniboiné—Montana, Alberta	Kiowa—Oklahoma
Blackfoot—Montana, Alberta	Mandan—N. Dakota
Cheyenne—Montana	Menomini—Wisconsin
Chipewyan—Hudson Bay to Lake Athabaska	Ojibwa—Great Lakes Region
Comanche—Colorado to Texas	Omaha—Nebraska
Cree (Plains)—Manitoba to Alberta	Osage—Missouri
Crow—Montana	Pawnee—Nebraska
Dakota (Santee)—Minnesota	Ponca—Nebraska
Dakota (Teton)—S. Dakota	Sarsi—Alberta
Fox—Wisconsin	Sauk—Wisconsin
Gros Ventre—Montana, Alberta	Shoshoni—Wyoming, Idaho
	Winnebago—Wisconsin

²³ Dorsey and Murie, *op. cit.*, p. 113. J. O. Dorsey, "Omaha Sociology," BAE, 3d Ann. Rept. (1884), pp. 233 f., 288, 321, 363; Fletcher and La Flesche, *op. cit.*, pp. 210, 279; James, *op. cit.*, chap. xii. Denig, *op. cit.*, p. 436; R. H. Lowie, *Societies of the Crow, Hidatsa and Mandan Indians*, AMNH, Anthropological Papers, XI (1913), 132-136.

Some Aspects of Political Organization Among the American Aborigines

Huxley Memorial Lecture For 1948

IN A GROSS DESCRIPTION OF CONTINENTAL AREAS THE AMERICAN aborigines figure as separatistic and democratic, contrasting in the former respect with the African Negro, in the latter with both African and Polynesian. The illuminating studies on African politics edited by Drs. Fortes and Evans-Pritchard have demonstrated decisively what readers of P. A. Talbot or Henri Labouret had long known, to wit, that the traditional picture of Negro government is over-simplified. To be sure, there have been many powerful monarchies in African history, but east of the Niger, in the Upper Volta region, and in the Anglo-Egyptian Sudan not a few tribes resist integration as much as any people in the world. In 1931 the 69,484 Lobi on French soil in the Upper Volta country were spread over 1,252 mutually independent sham villages (*prétendus villages*); a single one had over 600 residents, while 44 of these hamlets numbered fewer than 100, so that M. Labouret properly speaks of a *particularisme accusé*. Within no

Journal of the Royal Anthropological Institute of Great Britain and Ireland, LXXVIII (1948), 11-22.

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unit were there any chiefs, and assemblies convened to adjudicate particular issues had no means to execute their decisions. In short, the gamut of possible variations is realized in Negro Africa: we find there vast kingdoms on the pattern of Uganda and Benin, but also minute, headless, "anarchic" groups.¹

In the present essay I shall examine the corresponding phenomena in aboriginal America. In a discussion of this sort it is convenient, if not inevitable, to use such terms as "the State," "law," "government," "political," "sovereignty." Conforming to the views of Max Weber, Professor Radcliffe-Brown, and Professor Thurnwald as I understand them, I take these words to imply the control of physical force so far as a given society recognizes it as legitimate. Thus, the King of Uganda could rightfully order the execution of a subject, no matter how arbitrary the decree might seem from our point of view; and in West Africa the Mumbo Jumbo organization properly flogged malefactors. On the other hand, similar acts by the Ku Klux Klan are in usurpation of functions monopolized by the State in Western civilization.

However, a genetic view of political structure must reckon with the fact that primeval anarchy could not suddenly blossom forth into a modern State claiming absolute dominance within its territorial limits. It is, indeed, a documented fact that the states of the most advanced modern peoples did not develop contemporary pretensions until relatively recent times, yet their immediate antecedents did have a political organisation, in other words, laws and government. A simple society may be differentiated so as to foreshadow government, yet the coercive element may be lacking. The Yurok of north-western California and the Ifugao of Luzon have no chiefs or judges whatsoever, yet a dispute in their midst is settled by unofficial go-betweens approved by public opinion, who offer their services, though without an iota of authority. A logical dichotomy of societies on the rigid definition of Statehood indicated above would rule out

¹ Meyer Fortes and E. E. Evans-Pritchard, eds., *African Political Systems* (Oxford University Press, 1940); Henri Labouret, *Les Tribus du Rameau Lobi*, Université de Paris, Travaux et Mémoires de l'Institut d'Ethnologie, XV (1931), 56, 215, 386.

such phenomena as quite irrelevant to a study of government, but the common sense of comparative jurists regards them as highly significant. In the following inquiry, then, I shall indeed retain the exercise of force as the criterion of a full-fledged political organization, but I shall also consider what seem evolutionary stages toward that consummation.

The questions I ask concerning American Indians may be phrased as follows: Within what territorial limits does authority create some measure of solidarity? And what is the nature of the authority encountered? Specifically, where, in America, was a state of modern type realized? What trends can be discerned toward its evolution?

SEPARATISM AND INTEGRATION

Notwithstanding my initial qualifications, African systems on the whole do differ noticeably from those of the New World. According to Roscoe, the Baganda once numbered three million; by 1911 civil wars and the sleeping sickness had sadly reduced them, but not below the million mark. In 1668 Dapper credited Benin with a regular army of 20,000, which at a pinch could be increased to five times as many; the capital was five or six Dutch miles in circumference and had thirty main streets. In about 1870 Schweinfurth set the Shilluk at over a million; partly because of wars recent estimates are far more modest, yet they fluctuate between 50,000 and 100,000. Shortly before this explorer's visit a million Mangbettu had been under the sway of a single ruler. More recently the king of Ashanti had a quarter of a million subjects.²

Except in the few higher civilizations of Mexico, Yucatan, Colombia, and Peru, there is nothing to match even the least of these figures, apparent parallels proving deceptive. To be sure, aboriginal Chile is said to have been inhabited by from half a million to a million and a half Araucanians, but "there was no peacetime overall chief, no centralization of authority." There were, indeed, greater and lesser territorial units, but the subordination of the smaller "must have been close to purely nomi-

² John Roscoe, *The Baganda* (London, 1911), p. 6; P. A. Talbot, *The Peoples of Southern Nigeria* (Oxford University Press), pp. 162 f.; George Schweinfurth, *The Heart of Africa* (London, n.d.), I, 15, II, 35.

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nal." Only during the nineteenth century "the earlier atomistic peacetime political structure assumed somewhat greater unity, cohesion and hierarchization." To take a humbler figure, the 55,000 Navaho now rank as the largest native tribe in the United States. But, in the first place, theirs has been a mushroom growth: in 1868 they did not exceed 15,000—possibly not 9,000. Secondly, it is not clear that even this number were ever under a single government.³

As a matter of fact, a tendency to separatism was general. So advanced a people as the Hopi—some 3,000 in all—live in eleven villages, mislabelled "towns" by grandiloquent ethnographers. Yet even this paltry population neither has nor has had a common head: "between pueblo and pueblo there is an attitude of jealousy, suspicion and subdued hostility."⁴

Much ado has been made about the Creek Confederacy in the south-eastern United States and the Iroquois League of northern New York State. Unquestionably both prove wider political coöperation than was common in the New World, but their achievements must not be overrated. Authenticated occurrences reduce the cohesion involved in these alliances to a proper scale. It so happened that one of the Creek tribes, the Kasihta, became friendly with the alien Chickasaw. When the latter were at war with the Confederacy in 1793, "the Kasihta refused to take up arms with the other Creeks and their right to act in this independent manner was never questioned." Strictly parallel conduct among the federated Iroquois during the American Revolution was noted by Morgan. Each tribe was permitted to decide upon its course of action: the Oneida and half of the Tuscarora sided with the colonists, the other "leagued" tribes with the English. It was as though in 1914 Bavaria and half of Baden had joined the Allies to fight their fellow-Germans. Apart from this disintegration in a crisis, earlier claims on behalf of the League's influence have been exploded by Fenton's historical researches. The Iroquois did raid far and wide, but it hardly holds true that "their dominion was acknowledged from

³ John M. Cooper, "The Araucanians," *Handbook of South American Indians*, BAE, Bull. 143, Vol. II (1946), 694, 724; Clyde Kluckhohn and Dorothea Leighton, *The Navaho* (Cambridge, 1946), pp. xv, 73.

⁴ Mischa Titiev, *Old Oraibi*, PMH, Papers, Vol. XXII, No. 1 (1944), 59-68.

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Ottawa river to the Tennessee and from the Kennebec to Illinois River and Lake Michigan." In any case, at its peak in the seventeenth century the League never embraced over 16,000 or at most 20,000 persons.⁵

Since the one-eyed is king among the blind, the two faltering attempts at consolidation by the Creek and the Iroquois remain noteworthy "climactic" results, as my colleague Professor Kroeber might phrase it. In world perspective, however, they are unimpressive.

If skilful farming populations showed no greater sense of nationalism, little can be expected of the hunters. The Caribou Eskimo lacked permanent political units altogether, each community being in Professor Birket-Smith's judgment "an incoherent conglomerate of families or households, voluntarily connected by a number of generally recognized laws." The largest settlements have a population of about 50, and all of them jointly do not exceed ten times that figure. Earlier reports, to be sure, suggest a recent decline, due largely to famine, but even half a century ago the largest separate tribe of the area was not credited with over 178 souls. To turn toward the southern tip of the New World, the Ona population at its peak is set at between 3,500 and 4,000. Since this embraced 39 wholly independent territorial hordes, the average size of the political unit was about 100.⁶

Extreme as the Eskimo and the Fuegian instances may seem, they are paralleled on varying levels of cultural complexity. The exceptionally favourable food supply of North-west Californians failed to produce solidarity beyond the bounds of kinship and of immediate proximity. Of the seventeen independent Yurok hamlets listed in 1852, the largest had only 165 inhabitants; three others had over 100; five, well under 50.

Up and down the Pacific coast of North America similar con-

⁵ J. R. Swanton, "An Indian Social Experiment and Some of Its Lessons," *Scientific Monthly*, XXXI (1930), 368-376; J. N. B. Hewitt, "Iroquois," *Handbook of American Indians North of Mexico*, BAE, Bull. 30 (1907); William N. Fenton, "Problems Arising from the Historic Northeastern Position of the Iroquois," *Smithsonian Miscellaneous Collections*, Vol. C (1940); L. H. Morgan, *Ancient Society* (New York, 1877), Part II, chap. 5.

⁶ Kaj Birket-Smith, "The Caribou Eskimos," *Report of the Fifth Thule Expedition, 1921-1924* (Copenhagen, 1929), Vol. V, Part I, pp. 65-75, 260; Martin Gusinde, *Urmenschen im Feuerland* (Berlin, Wien, Leipzig, 1946), p. 97.

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ditions prevailed. In north-eastern Washington something less than 1,500 Sanpoil were spread over twenty villages, each of which, except for those conspicuously small, was autonomous. The Quinault, in the south-western part of the same state, probably numbered 800, divided among roughly 20 villages. The Lemhi of Idaho and associated Shoshoneans are set at 1,200 about the beginning of the nineteenth century, and this included more than a single group; Lewis and Clark estimated one group at 100 warriors and 300 women and children; another at 60 warriors. In eastern Brazil the Botocudo stock was split into several distinct tribes, some of them subdivided into bands from 50 to 200. Notwithstanding the existence of tribal chiefs, an authority reports "the constancy of their blood feuds, not only between distinct tribes, but even between bands of the same tribe." The Foot Indians of the Gran Chaco gathered in bands approximating the Botocudo pattern.⁷

No doubt an intermediate order of magnitude occurred. The Cheyenne of the Northern Plains at one time probably numbered not far from 4,000. Of the Ge stock, some members were inconsiderable enough: the recent Canella fluctuated about the 300 mark, but earlier travellers describe the villages of their congeners as rather larger. In 1824, for example, one Apinayé settlement had a population of 1,400; and the more remotely related Sherente display a sense of solidarity beyond the immediate local group. Though a paramount head is wanting, the several village chiefs sometimes jointly depose a grossly deficient colleague and appoint his successor. Characteristically, however, the Sherente have long been at bitter enmity with the Shavante, their closest linguistic and cultural kin.⁸

⁷ A. L. Kroeber, *Handbook of the Indians of California*, BAE, Bull. 78 (1925), p. 16; Verne F. Ray, *The Sanpoil and Nespelem: Salishan Peoples of Northeastern Washington*, University of Washington Publications in Anthropology, V (1932), 21-24, 109; Ronald L. Olson, *The Quinault Indians*, *ibid.*, VI (1936), 22; Julian H. Steward, *Basin-Plateau Aboriginal Sociopolitical Groups*, BAE, Bull. 120 (1938), pp. 188 f.; Curt Nimuendajú, "Social Organization and Beliefs of the Botocudo of Eastern Brazil," *Southwestern Journal of Anthropology*, II (1946), 97 ff.; A. Métraux, "Ethnography of the Chaco," *Handbook of South American Indians*, BAE, Bull. 143, Vol. I (1946), 302; *idem*, "The Botocudo," BAE, Bull. 143 (1946), p. 536.

⁸ K. N. Llewellyn and E. A. Hoebel, *The Cheyenne Way* (Norman, Okla., 1941), p. 78; Curt Nimuendajú, *The Apinayé*, The Catholic University of America, Anthro-

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Similar qualifications apply to the instances from the eastern United States. The League of the Iroquois has already been discussed. The Cherokee and the Choctaw were the two largest south-eastern tribes, being estimated at 22,000 and 15,000 souls, respectively, in 1650. However, once more the *political* unit is incomparably smaller than the linguistic. For the Choctaw, Swanton reasonably suggests some 40 to 50 synchronous communities "constituting small States, each with its chief." An anonymous French writer of *ca.* 1755 does speak of a *grand chef* of the nation, but adds that his authority was negligible. The Cherokee were scattered over at least 80 towns. "These people came under the domain of one tribal chief only in times of great emergency and then most imperfectly." On the whole, it seems likely that the figures set for the Natchez in 1650 and for the Powhatan in 1607—4,500 and 9,000—approach the limits attained within the area by any governmental entity.⁹

At this point it is well to recall the phenomenon luminously illustrated by Durkheim for Australians, by Mauss for the Eskimo, and since demonstrated elsewhere. The seasonal rhythm of life, rooted in economic exigencies, transforms the constitution of a group and, as a corollary, its social life. The consequences we shall consider later. For the present, we merely note that some of the figures quoted would hold only for a relatively brief portion of the year; at other times, the tribe breaks up into minute fragments in order more effectively to exploit the environment.¹⁰

To review the argument, American figures of a population approximating or exceeding 10,000 rarely, if ever, refer to permanently integrated political units.

How far does this conclusion apply to the four higher civilizations? As for the Aztec, the moot question of whether they totaled

pological Series, No. 8 (Washington, 1939), p. 7; *idem*, *The Serente*, Publications of the Frederick Webb Hodge Anniversary Publication Fund (Los Angeles, 1942), IV, 9 f.

⁹ W. H. Gilbert, *The Eastern Cherokees*, BAE, Bull. 133 (1943), p. 363; J. R. Swanton, *Source Material for the Social and Ceremonial Life of the Choctaw Indians*, BAE, Bull. 103 (1931), pp. 90, 95, 243; *idem*, *The Indians of the South-eastern United States*, BAE, Bull. 137 (1946), pp. 114, 123, 161, 175.

¹⁰ Emile Durkheim, *Les Formes élémentaires de la vie religieuse* (Paris, 1912); Marcel Mauss, "Essai sur les variations saisonnières des sociétés Eskimo: Étude de morphologie sociale," *L'Année sociologique*, IX (1906), 39-132.

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three or many more millions need not concern us; we are interested solely in what number belonged to the same state. That the hoary idea of an Aztec empire is untenable seems certain in the light of modern research. All we find is a belated league of three tribes which remained mutually distrustful: "the Aztecs had no sense of unity," no national spirit. Within the present limits of Mexico City, Tenochtitlan and Tlatelolco long persisted in complete independence of each other. At the time of the Spanish invasion the Texcocans joined the intruders against their former ally, Tenochtitlan. A quarter of a million people, or thereabouts, may possibly have had a single government on a strict definition.¹¹

Maya ruins are spread from northern Yucatan to Honduras, but they belong to different periods, and it is not easy to estimate the residents of any one state. Possibly in about A.D. 1000, according to legendary history, there was a league of three cities, of which Mayapan gained the ascendancy, establishing a centralized government two or three centuries later. This was followed by disintegration, leaving only petty chieftains for the Spaniards to contend with. In their era the rulers of Mani were "the most powerful in Yucatan." The tribute list for that province demonstrates 13,480 adult males. If we multiply this by six, or even ten, we still get no total population that looks spectacular by an African scale.¹²

The Chibcha numbered possibly a million, but they too were divided up among several distinct states, of which Zipa, the largest, is credited with 300,000 souls. The untrustworthiness of early estimates is indicated by a fantastic reference to armies of 50,000 whereas no more than 600 Zipa braves attacked the Spanish troops.¹³

In short, the solitary convincing instance of grandiose expansion in the Western Hemisphere is that of the Incas of Cuzco, Peru. Their realm did extend from Ecuador to northern Chile,

¹¹ G. C. Vaillant, *Aztecs of Mexico* (Garden City, 1941), pp. 91, 134, 213 f.

¹² A. M. Tozzer, *Landa's Relación de las Cosas de Yucatán*, PMH, Papers, XVIII, 64; Sylvanus G. Morley, *An Introduction to the Study of Maya Hieroglyphs*, BAE, Bull. 57 (1915), pp. 2-12; Ralph L. Roys, *The Book of Chilam Balam of Chumayel*, Carnegie Institution of Washington, Publ. 438 (1933), pp. 188-195.

¹³ A. L. Kroeber, "The Chibcha," *Handbook of South American Indians*, BAE, Bull. 143, Vol. II (1946), 887-909.

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embracing possibly 6,000,000 subjects. However, we must recollect that aggrandizement was a very late pre-Columbian achievement. "In early times neither the *Inca* nor any of their neighbours thought of organizing their conquests as a permanent domain." Until the reign of Pachacuti (*ca.* A.D. 1438) "towns very near to Cuzco preserved complete freedom of action and raided one another's territory whenever there seemed to be a good opportunity for plunder."¹⁴

With a unique exception, then, the American Indians must be regarded as eminently separatistic.

However, there was certainly no sudden mutation from an Ona-like to an Inca-like condition. The Creek and the Iroquois schemes indicate a stage of solidarity, however imperfect, on a larger than normal scale. Still more illuminating are phenomena within the historic period. Whereas the two well-known leagues united mainly communities of like or closely related speech, Pontiac (1763) and Tecumseh (died 1813) brought together wholly unconnected tribes. The Ottawa chief rallied not merely his own people and their Algonkian congeners, but also the Seneca and the Wyandot of Iroquoian stock and the Siouan Winnebago. The Shawnee leader arrayed Algonkians, Wyandot, and even Creek Indians against the United States. Though both uprisings proved abortive, though they culminated in negation of British and American overlordship rather than in the creation of a close-knit aboriginal state, they do prove that under strong emotional stimulus exceptional natives could and did visualize coöperation of major scope. Individuals of comparable organizing skill, however diverse their motivation, must be credited with the nascent forms of Andean imperialism.¹⁵

COERCIVE AUTHORITY

I now turn to my second theme—the manifestation or adumbration of coercive authority in aboriginal America. As in Africa, so here too, the range of observable phenomena is very great. At one extreme we find the "anarchic" Eskimo, north-west Califor-

¹⁴ John H. Rowe, "Inca Culture at the Time of the Spanish Conquest," *ibid.*, pp. 184 f., 201–209, 257 ff.

¹⁵ James Mooney, "The Ghost Dance Religion and the Sioux Outbreak of 1890," *BAE, 14th Ann. Rept.* (1896), pp. 681–691.

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nians, and Fuegians; at the other, the Incas of Peru. But in the New World, the latter must be regarded as atypical, and an intermediate condition represents the norm. By this I mean a condition with differentiation of one or more individuals as headmen, even though their actual power is circumscribed or even negligible. For convenience of exposition I shall call these officials "titular chiefs" in contrast to the "strong chiefs" possessing unquestioned authority. After discussing the functions of these two types of civil heads, I shall examine the factors that may have strengthened the titular chief's hands in the American milieu; and I shall likewise consider what agencies aside from chiefs of either category have assumed State functions.

Titular chiefs.—Titular chiefs vary considerably in actual status. The Chipewyan individuals who bear the title exercise so little influence apart from the accident of personality that one might perhaps just as well put this north Canadian tribe into the chiefless category with the Eskimo and the Fuegians. Elsewhere the office is not only honorific, but also fraught with definite public functions. In order to overcome semantic difficulties it will be best to emphasize what the titular chief is *not*, before trying to indicate his positive attributes. That he cannot, in many American societies, correspond to an African chief is apparent whenever a single band or tribe has more than one title-bearer. Three hundred Canella are headed by three "chiefs"; another Ge people, the Pau d'Arco Kayapo, generally had two; the related Gorotire band, five (in 1940). Until 1880 the Omaha had two principal chiefs, with a varying number of lesser ones; this oligarchy was then superseded by a septet of uniform rank. Among the Arapaho there were four chiefs, and the Cheyenne with a population never greatly exceeding 4,000 had forty-four!¹⁶ A series of examples from diverse culture areas will elucidate what American chiefs typically lacked.

The Ojibwa (round Lake Superior) had a council "with vague and limited powers." It selected a chief "whose power was even vaguer than that of the council," and who was "less able to work his will against an existing custom." Tanner, who lived

¹⁶ Birket-Smith, *op. cit.*, p. 66; J. O. Dorsey, "Omaha Sociology," BAE, 3d Ann. Rept. (1884), p. 357; Curt Nimuendajú, "A Note on the Social Life of the Kayapó," *American Anthropologist*, XLV (1943); Llewellyn and Hoebel, *op. cit.*, pp. 67 ff.

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in this region from 1789 until 1822, mentions "the unstable power and influence of the chiefs." In an assembly of 1,400 Assiniboiné, Cree, and Ojibwa, he remarks, "not one would acknowledge any authority superior to his own will." A chief was, indeed, entitled to some deference, "but this obedience . . . continues no longer than the will of the chief corresponds entirely with the inclination of those he leads." About the same time the trader Tabeau notes that among the Teton Dakota "all authority is as naught before the opposition of a single individual," and for the related Assiniboiné, Denig—himself the husband of a woman of that tribe—offers an eye-witness's priceless corroboratory evidence. At a council attended by him the "leading chief" advocated peace with the Crow; a tribesman of lesser dignity vigorously and successfully opposed the idea, carrying the assembly with him. The historian Parkman, on the basis of personal experience in 1846, declares that very few Oglala Dakota "chiefs could venture without instant jeopardy of their lives to strike or lay hands upon the meanest of their people" and correctly notes the paradox that the "soldiers," i.e., police, "have full license to make use of these and similar acts of coercion." This institution will be discussed later. Among the Shoshoneans of Nevada, "any family was at liberty to pursue an independent course at any time"; in Arizona the head of the Maricopa had functions "more admonitory than coercive"; and among the Yuma the tribal leader, though appealed to in a dispute, was "more significant as an embodiment of spiritual power than as a lawgiver or executive." Equivalent testimony comes from Oregon and Washington.¹⁷

¹⁷ William Jones, "Central Algonkian," *Annual Archeological Report*, 1905 (Toronto, 1906), p. 137; Edwin James, ed., *An Indian Captivity: John Tanner's Narrative of His Captivity among the Ottawa and Ojibwa Indians*, Sutro Branch, California State Library, Occasional Papers, No. 20 (San Francisco, 1940), p. 151; Anne Heloise Abel, ed., *Tabeau's Narrative of Loisel's Expedition to the Upper Missouri* (Norman, Okla., 1939), pp. 105 f.; Edwin T. Denig, "Indian Tribes of the Upper Missouri," *BAE, 46th Ann. Rept.* (1930), pp. 430-456; Francis Parkman, *Prairie and Rocky Mountain Life* (Columbus, 1856), p. 291; Leslie Spier, *Klamath Ethnography*, Univ. Calif. Publ. Amer. Arch. and Ethn., XXX (1930), 33; *idem*, *Yuman Tribes of the Gila River* (Chicago, 1933), p. 158; Ray, *op. cit.*, p. 111; Steward, *op. cit.*, pp. 246-260; Grenville Goodwin, *The Social Organization of the Western Apache* (Chicago, 1942), pp. 178 f.; C. D. Forde, *Ethnography of the Yuma Indians*, Univ. Calif. Publ. Amer. Arch. and Ethn., XXVIII (1931), 134 f.

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Superficially the stratified societies of coastal British Columbia are different, but only superficially in the questions at issue. What they emphasize is social eminence, not political power. A Haisla chief "gives orders only in matters directly concerned with feasts and potlatches,"—not in cases of quarrels, theft, or murder; the Tsimshian equivalent was responsible for his followers' safety in battle and indemnified the mourners if their kindred had been killed. How different from an African potentate who owns his subjects' bodies and collects all damages for injuries sustained by them.¹⁸

South America yields corresponding testimony. In British Guiana a Barama headman has limited authority. Each of the three Canella dignitaries works like everyone else; none of them wears a badge of higher status, or interferes in private affairs, or issues commands, or imposes penalties. Among the related Apinayé, the headman does initiate measures against a sorcerer, but he cannot order an execution without popular assent. To take two more Brazilian examples, Karaya villagers simply desert a chief whose actions they resent; and though a Nambikuara leader enjoys a good deal of influence, he "has no coercitive power at his disposal." In short, the typical American chief may enjoy social standing, but he lacks sovereignty.¹⁹

What, then, are the titular chief's positive attributes and functions? The outstanding one forthwith explains the deficiency I have harped on: he refrains from attempting physical force, because many societies conceive him as primarily a peacemaker. It would be a contradiction in terms for him to mete out punishment when his business is to smooth ruffled tempers, to persuade

¹⁸ Edward Sapir, "The Social Organization of the West Coast Tribes," *Transactions of the Royal Society of Canada*, Sec. II, 3d ser., Vol. IX (1915); R. L. Olson, *The Social Organization of the Haisla of British Columbia*, Univ. Calif., Anthropological Records, II (1940), 182; Franz Boas, "Tsimshian Mythology," *BAE*, 31st Ann. Rept. (1916), pp. 429 ff., 499.

¹⁹ John Gillin, *The Barama River Caribs of British Guiana*, PMH, Papers, XIV, No. 2 (1936), 98, 140; Curt Nimuendajú, *The Apinayé*, pp. 19 f., 131 f.; *idem*, *The Eastern Timbira*, Univ. Calif. Pub. Amer. Arch. and Ethn., XLI (1946), 93, 159–162, 239 f.; Fritz Krause, *In den Wildnissen Brasiliens* (Leipzig, 1911), p. 321; Claude Lévi-Strauss, "The Social and Psychological Aspects of Chieftainship in a Primitive Tribe: The Nambikuara of North-Western Mato Grosso," *Transactions of the New York Academy of Sciences*, Ser. II, No. 1 (1944), p. 23.

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the recalcitrant, coax and even bribe the justly aggrieved into forgoing vengeance. He is, indeed, a go-between of the Yurok or Ifugao order, but with the essential difference of being the official, recognized, permanent moderator instead of a self-appointed one *ad hoc*. In order to compass his end—maintenance of communal harmony—he might stoop to eating humble-pie and to personal sacrifices. A Sanpoil chief presents each litigant with a blanket; his Cree colleague is expected to give up thoughts of revenge on his own behalf, such as other men freely indulge. A Winnebago went still further: "If necessary, the chief would mortify himself, and with skewers inserted in his back have himself led through the village to the home of the nearest kinspeople of the murdered person." By thus arousing compassion he hoped to avert a feud.²⁰

No wonder that an appeaser *ex officio* was not associated with warfare, was often—in his official capacity—deliberately divorced from violence and discipline. An Iroquois sachem's duties, Morgan reports, "were confined to the affairs of peace. He could not go out to war as a sachem." His position was sharply separated from the military leader's, being hereditary in the clan, whereas a successful captain gained a "chiefly" title of another category by personal bravery. This polarity was widespread. In a Fox Indian (Wisconsin) council, the Quiet and the War Chief were complementary figures, as are the Pueblo Town and War Chiefs—the former being prescriptively a man of peace who must not even go hunting, the latter a policeman who threatens punishment. The Omaha neither let a chief head a raid nor even allowed him to serve as a subordinate officer of one. Again, "a man who has often been on the warpath," say the Pawnee, "becomes imbued with the desire to take scalps and capture ponies and is no longer fit to be chief." A Winnebago chief always belongs to one clan, a policeman to another.²¹

²⁰ David G. Mandelbaum, *The Plains Cree*, AMNH, Papers, XXXVII (1940), 222; Paul Radin, "The Winnebago Tribe," BAE, *37th Ann. Rept.* (1923), p. 209.

²¹ Morgan, *op. cit.*, Part II, chaps. ii, iv, v; William Jones, *Ethnography of the Fox Indians*, BAE, Bull. 125 (1939), p. 82; Titiev, *op. cit.*, pp. 59–68; Elsie Clews Parsons, *Pueblo Indian Religion* (Chicago, 1939), pp. 154 f.; J. O. Dorsey, "Omaha Sociology," BAE, *3d Ann. Rept.* (1884), p. 217; G. A. Dorsey and J. R. Murie, *Notes on Skidi Pawnee Society*, Field Museum of Natural History, Anthropological Ser., XXVII (1940), 112 f.

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This dichotomy prevails even where a fusion of civil and military preëminence seems at first blush easily realized. In several South American tribes the "chief" did lead war parties, but whereas he became a virtual autocrat on a raid he relapsed into his usual impotence on his return. On this point early sources on the Kariri and the Tapuya (eastern Brazil) agree with recent ones on the Taulipang (south of the Roroima) and the Jivaro (Ecuador). One North American phenomenon is instructive in this context. The Iroquois League found it desirable to create two generals "to direct the movements of the united bands," but these officials never aspired to a dictatorship. To quote Morgan, "the essential character of the government was not changed. . . . Among the Iroquois this office never became influential."²²

In short, the conceptions of civil and of military leadership were distinct in America. There was sporadic tyranny even in the democratic Northern Plains societies, but it sprang from individual bullying, usually supported by a powerful body of kin or from putative supernatural sanction, not from the coup d'état of a captain returning drunk with success and filled with the ambition of a despot.

Besides being a skilful peacemaker, the ideal chief was a paragon of munificence. This may hold more often in North than in South America, but instances are not wanting in the south. Thus, a Nambikuara headman constantly shares with his tribesmen whatever surplus of goods he may have acquired: "Generosity is the quality . . . which is expected of a new chief." In the north, this demand is constant. In Alaska, where the Eskimo were affected by the ideology of their Indian neighbours, the title of "chief" automatically devolved on that Nuniwak who entertained most lavishly at village feasts. A chief of the Tanaina Athabaskans (about Cook Inlet) feeds and clothes the destitute, provides for the households of men away on hunt-

²² Theodor Koch-Grünberg, *Vom Roroima zum Orinoco* (Stuttgart, 1923), III, 94; Martin de Nantes, *Relation succincte et sincère de la mission du père Martin de Nantes* (Paris, 1706), p. 103; Thomaz Pompeu Sobrinho, "Os Tapuias do Nordeste e a monografia de Elias Herckman," *Revista do Instituto do Ceará*, XLVIII (1934), 18; Rafael Karsten, *Blood Revenge, War, and Victory Feasts among the Jibaro Indians of Eastern Ecuador*, BAE, Bull. 79 (1923), pp. 7 f.; Morgan, *op. cit.*, Part II, chap. v.

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ing trips, adopts orphans, and even pays for shamanistic services that are beyond a poorer tribesman's means. The coastal tribes of British Columbia, notwithstanding their emphasis on hereditary status, insisted that a headman should validate his claims by frequent distribution of property. In the Plains area, chieftainship and niggardliness were mutually exclusive. To quote Wissler, "no Blackfoot can aspire to be looked upon as a headman unless he is able to entertain well, often invite others to his board, and make a practice of relieving the wants of his less fortunate band members." The Cheyenne or the Crow had identical standards of behaviour.²³

A third attribute of civil leadership is the gift of oratory, normally to be exercised on behalf of tribal harmony and the good old traditional ways. Speaking of the Sherente, Nimuendajú reports:

On many evenings . . . I saw the chief assemble the village. Stepping in front of the semi-circle . . . , he would impressively and vividly harangue the crowd for possibly an hour. Usually he began circumstantially explaining the half-forgotten ceremonial of some festival. . . . There followed a lengthy admonition . . . to preserve ancient usage. In conclusion, he would urge all to live in peace and harmony. . . .

The extinct Tupinambá of coastal Brazil regarded a species of falcon as the king of his zoölogical class: "ils se fondaient sur le fait que cet oiseau se levait de bon matin et haranguait les autres oiseaux, tout comme le chef de la hutte le faisait chaque jour, à l'aube, dans les villages tupinamba." In the Chaco the contemporary Pilaga merely postpone oratory until nightfall: "Ce prurit d'éloquence est commun à tous les caciques et constitue . . . un des principaux attributs de leur dignité. . . . Le thème habituel de ces harangues est la paix, l'harmonie et l'honnêteté, vertus recommandées à tous les gens de la tribu." In characteristic fashion a Chiriguano explained to Nordenskiöld the ex-

²³ Lévi-Strauss, *op. cit.*, p. 24; Margaret Lantis, "The Social Culture of the Nunivak Eskimo," *Transactions of the American Philosophical Society*, n.s., Vol. XXXV (1946), Part III, p. 248; Cornelius Osgood, *The Ethnography of the Tanaina*, Yale University Publications in Anthropology, No. 7 (1937), p. 132; Sapir, *op. cit.*; Clark Wissler, *The Social Life of the Blackfoot Indians*, AMNH, Anthropological Papers, VII (1911), 23; Llewellyn and Hoebel, *op. cit.*, p. 79.

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istence of a female head of the tribe: her father had taught her to speak in public. Thousands of miles to the north, in the Shoshone vernacular a headman figures as "the talker," which "designates his most important function." Maricopa and Apache chiefs, too, were matutinal lecturers; and among the Havasupai (Arizona) Spier says: "it might be said not that a chief is one who talks, but that one who talks is a chief."²⁴

In my opinion, then, the most typical American chief is not a lawgiver, executive, or judge, but a pacifier, a benefactor of the poor, and a prolix Polonius.

Strong chiefs.—But not all chiefs were only titular. A relatively small, but significant, number of societies had genuine rulers. It is best to begin with an unexceptionable example, the Inca state, the outstanding American sample of Drs. Fortes and Evans-Pritchard's category A—political systems with a well developed governmental apparatus.

The Inca emperor, ruling by divine right, undoubtedly did control means of coercion. Through an elaborate "bureaucracy" he exacted tribute from his subjects and directed their labours, even their private lives. He did not scruple to transfer masses of the population from one province to another in the interests of the dynasty. What elsewhere in the New World were private wrongs here became offences against the Crown and called for summary official penalties.

Emblematic of autocracy were the trappings of royalty otherwise conspicuously rare in America. The ruler wore and carried impressive regalia, travelled in a litter borne by special attendants, kept a large harem, and surrounded his court with an elaborate etiquette. His corpse was prepared for preservation in the palace, and his favourite wives together with a suitable retinue were strangled to accompany their master to the hereafter.²⁵

Concerning the Aztec chief the authorities yield contradictory and confusing evidence, but it seems clear that he did not con-

²⁴ Métraux, *La Religion des Tupinamba*, p. 179; *idem*, "Études d'Ethnographie Toba-Pilaga," *Anthropos*, XXXII (1937), 390; Erland Nordenskiöld, *Indianerleben* (Leipzig, 1912), p. 229; Steward, *op. cit.*, p. 247; Leslie Spier, *Havasupai Ethnography*, AMNH, Anthropological Papers, XXIX, Part 3 (1928), 237 f.; *idem*, *Yuman Tribes of the Gila River* (Chicago, 1933), p. 158; Goodwin, *op. cit.*, pp. 165 f., 178.

²⁵ Rowe, *loc. cit.*

form to the Inca pattern. He was apparently not closely identified with the supreme deity; and, notwithstanding fixed succession within a lineage, he could be deposed. The hereditary *halach-uinic* of the Maya probably wielded greater power, claiming tribute as well as military service and periodically examining subordinate chiefs in order to weed out pretenders. Significantly, both he and the sacred war leader travelled in a litter, a symbol of exalted rank also attached to a Chibcha monarch, who resembled his Peruvian parallel in other respects. He, too, received tribute, kept a seraglio, hedged himself about with ceremonials, and was buried with several wives and slaves. When he expectorated, an attendant caught the spittle in an extended cloth—a form of flunkeyism hardly conceivable among the Crow or Cheyenne.²⁶

It may be natural to find a full-blown political system among the materially advanced populations whose very numerical strength requires some central control if there is to be any solidarity. But, interestingly enough, the outlines of such a system appear also in the tiny states of the south-eastern culture area of North America. This anomaly has been recently stressed by Steward. Indeed, the Natchez sovereign came very close to the Inca conception of royalty. He claimed relationship with the solar deity, his kinsmen ranking as "Little Suns"; held power over life and death; travelled in a litter; and in death was followed by wives and servants, his bones being laid to rest in a temple near those of his predecessors. His subjects were obliged to keep at least four paces away from his person and would hail him "with genuflections and reverences." Elements of this complex, such as the litter, characterize the Timucua of Florida and the Chickasaw of Northern Mississippi; and though the monarchical principle is generally weaker in the south-east as a whole than among the Natchez, it reappears in full force in Virginia. "As halfe a God they esteeme him," Captain John Smith reports in writing of the Powhatan chief. This ruler arbitrarily ordered his subjects to be beaten, tortured, and killed, and kept a sizeable bodyguard to execute his will. "What he

²⁶ Roys, *op. cit.*, pp. 192 f.; Tozzer, *op. cit.*, pp. 165, 222; Kroeber, *The Chibcha*, p. 946; Vaillant, *op. cit.*, pp. 113 ff.

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commandeth they dare not disobey in the least thing." He demanded tribute of skins, beads, corn, and game; and numerous concubines waited upon him. Here and there undemocratic usages turn up as far north as New England, where they have been plausibly ascribed to south-eastern influences. However that be, the specific resemblances among Peruvians, Natchez, and Powhatan suggest a common origin for so atypical an American polity.²⁷ Of course, this does not imply that the social scheme diffused from the Inca Empire itself, a chronologically impossible assumption, but rather that certain elements of a monarchical system crystallized somewhere between Yucatan and Peru and spread in a period considerably antedating the expansion of Inca sovereignty. If I understand Professor Steward correctly, this agrees with his recent interpretation of the facts.

Given the marked libertarian bias of most American aborigines, how can we conceive the growth of absolutism? What could convert the titular chief who cajoled his tribesmen into preserving the social equilibrium into a veritable king?

Evolutionary germs.—In reëxamining the chieftless or virtually chieftless tribes we discover here and there that the Indians willingly subordinate themselves to some individual for a particular enterprise. In a rabbit drive the Washo and neighbouring Shoshoneans of the western Basin temporarily followed a leader noted for his skill as a hunter, though "apart from that special occasion his authority was nil."²⁸

An exceptionally large gathering may favour the similarly spontaneous acceptance of a director. The Yahgan, who normally move about in very small groups, unite up to the number of eighty when a beached whale provides food for the participants at an initiation ceremony. Without an election some mature man

²⁷ J. R. Swanton, *Indian Tribes of the Lower Mississippi Valley and Adjacent Coast of the Gulf of Mexico*, BAE, Bull. 43 (1911), pp. 100–110, 139 ff.; *idem*, *The Indians of the South-Western United States*, pp. 161, 175, 598 ff., 641–654, 728, 730; Regina Flannery, *An Analysis of Coastal Algonquian Culture*, The Catholic University of America, Anthropological Series, No. 7 (1939), pp. 116 f., 122 f.; J. H. Steward, "American Culture History in the Light of South America," *Southwestern Journal of Anthropology*, III (1947), 97.

²⁸ R. H. Lowie, *Notes on Shoshonean Ethnography*, AMNH, Papers, XX (1924), 196 f., 284 f., 305.

well posted in traditional usage emerges as the master of ceremonies and henceforth plans the daily routine. What is more, he appoints a constable, who in turn chooses a number of deputies. These policemen exercise genuine legal authority: they forcibly drag refractory tyros to the initiation lodge, overpower a troublemaker, bind him, and let him lie for half a day without food or drink. The Yahgan, furthermore, have a men's club: the members as a whole bully their wives into fetching fuel and food for the assemblage, and one man has the duty of keeping women from prying.²⁹

Informally established offices are not necessarily ephemeral. The Nambikuara illustrate the rise of a relatively stable chieftaincy, as suggestively described by Lévi-Strauss. A man with inborn gifts of leadership forms the nucleus for a group that voluntarily acclaims him, thereby shifting responsibility to his shoulders. He directs the food quest during the difficult dry season, shares his surplus freely, prepares arrow poison for his adherents, and plans their entertainments. In requital, they concede him certain prerogatives, such as plural marriage, but without their approbation he is powerless. Here, then, there emerges a titular chief with genuine influence, though still not a ruler.³⁰

By way of contrast there is a short-lived but absolute authority of the war leader as already noted for several South American groups. For North American parallels we have fuller data. A Crow supposedly organized his raid only when prompted by a supernatural patron, whence the leader's ascendancy over all who joined his expedition: theirs were the menial tasks, his the loot to dispose of as he chose, but also the responsibility for failure and losses. The equalitarian attitudes of everyday life recede, supplanted by a transitory overlordship. Omaha captains even appointed policemen who had the right to beat refractory or lagging warriors. Fleeting dictatorship of this limited range is not irrelevant to our problem. About 1820 the Cheyenne conceived themselves as one huge war-party, whose leader thus automatically became supreme, supplanting the tribal council of

²⁹ Martin Gusinde, *Die Feuerland Indianer, II: Die Yamana; vom Leben und Denken der Wassernomaden am Kap Hoorn* (Mödling bei Wien, 1937), pp. 199-208, 653, 779 ff., 798 ff., 805-961, 1319-1376.

³⁰ Lévi-Strauss, *op. cit.*, pp. 21 ff.

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"chiefs." Yet in consonance with native ideology he retained not a vestige of his special authority when his task was done.³¹

Undisputed supremacy for a restricted period was also granted during religious festivals. When a Hopi ceremony is in process, Stephen learned, "the chief of it is chief of the village and all the people." Similarly, the priest who directed a Crow Sun Dance was not merely the master of ceremonies, but the temporary ruler of the tribe, superseding the camp chief.³²

Non-chiefly authority.—Perhaps the most remarkable instances of authority, full-fledged and not altogether ephemeral, turn up in connection with important economic undertakings which are to be safeguarded in the common interest.

A pertinent phenomenon from northern Brazil seems to have eluded general notice. The Apinayé chief, if properly qualified, succeeds his maternal uncle in the office, by virtue of which he guards the villagers' interests and orders the execution of evil sorcerers. But at the planting season a pair of men representing the moieties begin to act as independent executives. One of them collects the seeds, invokes the Sun to prosper them, and is the first to plant a plot. Both of these officials watch the crops, chant daily songs to promote growth, and *forcibly prevent or punish premature harvesting*. "Woe to any Indian woman who should dare to remove clandestinely even the most trifling product from her own plots before maturity is officially announced!" If the rule is broken, they "attack the houses of the villages or the camp, raging and throwing everything about pellmell, breaking the vessels and flogging with thorny whips any women who have not fled in good season, or gash them with a special weapon. . . ." Even the chief's wife was once severely chastized for transgressing the law. Apart from the religious feature, the phenomenon reminds an Americanist of the Winnebago or Menomini constables who punished overhasty gatherers of wild rice.³³

The last-mentioned officers from the Woodlands of North

³¹ J. O. Dorsey, *op. cit.*, p. 321; Llewellyn and Hoebel, *op. cit.*, p. 163.

³² A. M. Stephen, *Hopi Journal*, ed. by Elsie Clews Parsons, Columbia University, Contributions to Anthropology, Vol. XXIII, Parts 1 and 2 (1936), 728.

³³ Nimuendajú, *The Apinayé*, pp. 13, 19, 89, 131 f.; Paul Radin, "The Winnebago Tribe," BAE, 37th Ann. Rept. (1923), pp. 226 f.; Alanson Skinner, *Social Life and Ceremonial Bundles of the Menominee Indians*, AMNH, Anthropological Papers, XIII (1913), 26.

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America are obvious variants of the familiar Plains Indian "soldiers" mentioned by Parkman. Their activities developed most spectacularly during a communal hunt, upon whose outcome the very life of the natives would depend. In order to ensure a maximum kill, a police force—either coinciding with a military club, or appointed *ad hoc*, or serving by virtue of clan affiliation—issued orders and restrained the disobedient. In most of the tribes they not only confiscated game clandestinely procured, but whipped the offender, destroyed his property, and, in case of resistance, killed him. The very same organization which in a murder case would merely use moral suasion turned into an inexorable State agency during a buffalo drive. However, Hoebel and Province have shown that coercive measures extended considerably beyond the hunt: the soldiers also forcibly restrained braves intent on starting war parties that were deemed inopportune by the chiefs; directed mass migrations; supervised the crowds at a major festival; and might otherwise maintain law and order.³⁴

Here, then, we find unequivocal authoritarianism. Theoretically, the police acted, at least in a number of tribes, under the direction of the tribal chief or council. The foundation was thus laid for either an autocracy or an oligarchy. Why did this logical end fail to be consummated?

In the first place, let us revert to the seasonal rhythm of the Plains Indians. During a large part of the year the tribe simply did not exist as such; and the families or minor unions of families that jointly sought a living required no special disciplinary organization. The soldiers were thus a concomitant of numerically strong aggregations, hence functioned intermittently rather than continually.

³⁴ Wissler, *The Social Life of the Blackfoot Indians*, pp. 22–26; *idem*, *Societies and Ceremonial Associations in the Oglala Division of the Teton-Dakota*, AMNH, Anthropological Papers, XI (1912), 17, 24; *idem*, *The American Indian* (New York, 1922), pp. 161, 178; Jane Richardson, *Law and Status among the Kiowa Indians*, American Ethnological Society, Monographs, I (1940), 9 f.; Diamond Jenness, *The Sarcee Indians of Alberta*, National Museum of Canada, Bull. 90, Anthropological Series, No. 23 (1938), pp. 11, 41; D. G. Mandelbaum, *op. cit.*, pp. 203, 205; A. L. Kroeber, *Ethnology of the Gros Ventre*, AMNH, Anthropological Papers, I (1908), 147 f.; E. A. Hoebel, "Associations and the State in the Plains," *American Anthropologist*, XXXVIII (1936), 433–438; *idem*, *The Political Organization and Law-Ways of the Comanche Indians*, AAA, Memoirs, No. 54 (1940), p. 82.

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Secondly, the "constitutional" relationship of chief and police was by no means so simple as might appear. It was definitely not that of the head of a modern state toward his army. Denig, whose observations on the impotence of Assiniboine chiefs have been quoted, ascribes to the police "the whole active power of governing the camp or rather of carrying out the decrees and decisions of the councils." He himself witnessed "two killed and many severely thrashed for their misdemeanours." Were the soldiers, then, strictly subordinate to the *council*, as Denig's phraseology implies? Well, according to the same authority, if councillors threatened to grow violent at a meeting, "two soldiers advanced to the middle of the lodge and laid two swords crosswise on the ground, which signal immediately restored order and quiet." There was thus a dispersal of sovereignty: the titular chief had none, the council was in principle a governing board controlling a police squad that carried out their decisions, but *de facto* the theoretically subordinate police acted with considerable independence.³⁵

The much fuller data on the Cheyenne collected by Messrs. Llewellyn and Hoebel corroborate this interpretation.³⁶ Here a self-perpetuating council of forty-four "chiefs" with safe tenure during a ten-year term of office was headed by five priest-chiefs, one of whom took precedence as the representative of the mythical culture hero, Sweet Medicine. This did not make him the equivalent of a Shilluk king, for he "wielded no consequent special political authority" nor was he above the traditional law. Unlike other Plains peoples, the Cheyenne for ritual reasons conceived homicide as a crime. When Little Wolf, the head chief and a man of superb record, killed a tribesman, though under mitigating circumstances, he did not escape the penalty, but went into voluntary exile. A lesser chief is known to have been severely flogged by the soldiers for a similar offence and was likewise banished, though not demoted in rank.

To turn to the council as a whole, it is true that they appointed one of the five existing military clubs to oversee a migration or a communal hunt. But, apart from such matters as

³⁵ E. T. Denig, *op. cit.*, pp. 436, 439, 442, 444 f., 448, 455, 530 ff.

³⁶ Llewellyn and Hoebel, *op. cit.*, pp. 67-131.

directing travel, the "chiefs" were little concerned with secular affairs, sometimes waiving the right to a definitive decision and thus leaving a great deal to the discretion of their appointees. Accordingly, the police became the final authority in a large number of issues either beyond the competence of their electors or deliberately turned over to them by the council for settlement. The soldiers thus could, and repeatedly did, inaugurate legal precedents, nor does it appear that these were ever challenged by the "chiefs."

Llewellyn and Hoebel draw attention to an extraordinary illustration of police autonomy. During a march directed by the Fox society, a councillor named Sleeping Rabbit answered a taunt by shooting the interlocutor, a member of the Dog organization. The arrow could not be extricated. The Foxes severely mauled and kicked the criminal; and when the victim's arm grew worse they decreed that Sleeping Rabbit must amputate it, a novel verdict. Public sentiment, crystallized in the four other societies, favoured exiling the culprit, but he avowed his guilt and, in self-infliction of a fine, presented the Foxes with five good horses. This settled the matter.

As our authorities show, this was emphatically not an example of composition. Damages accrued neither to the victim nor to his kin nor to his society, but to the Foxes. *They* were the State in this case, receiving the indemnity as a Bantu ruler might in corresponding circumstances. Of course, so far as we know, the case is unique and might have remained so throughout Cheyenne history; but the mere possibility of its occurrence is significant.

The relations of the Cheyenne council and soldiers were, of course, determined by the general American conception of chieftaincy. If more than temporary sovereignty were to be attained at all, it would thus more naturally centre in the police. Here we encounter a third factor that militated against autocracy or oligarchy. In this culture area the constabulary force was rarely fixed, being as a rule recruited differently for different seasons or even for specific occasions. In a Pawnee village, for example, the chief's adjutant and three of his deputies acted as police, but for a buffalo hunt a priest chose one of four

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societies as a nonce police.³⁷ The Cheyenne, we have noted, had five such organizations; it was not likely that four of them would calmly submit to the oligarchical pretensions of one rival body.

It so happens that in this tribe the Dogs did enjoy an unusual advantage over the other clubs: by an accident of history, a century or more ago, the males of one band collectively joined this society, so that in this solitary instance society and band coincided in adult male membership. The chief of the Dogs was thus *ipso facto* head of his band, and the Dog men remained united during the winter when rival clubs were scattered over various local divisions. Here, then, the germ for hegemony occurred, but it never reached fruition.

A further point must be mentioned. Within any one of the military clubs its chief was supreme, issuing orders like a war captain and sometimes ruling his members with an iron hand. Yet the libertarian impulses of these Indians would not brook servility in an absolute sense; in 1863, characteristically, the Dogs *forbade* their chief to attend a treaty council with American commissioners!

In short, though the Plains Indians indubitably developed coercive agencies, the dispersal of authority and the seasonal disintegration of the tribes precluded a permanent State of modern type. Generalizing for the whole of America, there were sundry gropings towards centralization of power, but counter-acting trends made them fall short of permanent results. Yet such results were achieved in Peru and in so relatively simple a setting as that of the Powhatan. What were the circumstances involved in these cases? And is it possible to detect similar factors in the normally libertarian societies?

The religious factor.—When Alexander the Great aspired to imperial grandeur, he was not content with the glory of a successful general, but claimed divinity and, as a mark of its acceptance, prostration. This sacred character, we have seen, supposedly belonged to the Inca ruler and to the Natchez Great Sun; the obeisances and genuflections in their presence are the equivalent of Alexander's demand for *proskúnésis*. With frankly

³⁷ Dorsey and Murie, *op. cit.*, p. 113.

evolutionary aim I shall assemble some data from the simpler American tribes in order to show that religious beliefs were used to attain political influence there; and I suggest that the awe which surrounded the protégé of supernatural powers formed the psychological basis for more complex political developments. It is possible for a titular chief to add to his standing by combining spiritual blessings with civil eminence, or he may enter an alliance with the religious functionary, thus foreshadowing the familiar spectacle of State and Church joined in the support of the established order.

The latter contingency is classically exemplified in Gayton's study on the Yokuts,³⁸ a Central Californian stock of some 18,000 souls divided into over fifty autonomous tribelets probably never exceeding 800 in population. In each of these units an acceptable member of the Eagle lineage served as chief, representing the mythical Eagle who had ruled the world in dim antiquity. Notwithstanding this lofty role, the chief was not an autocrat, but he did hold more than nominal precedence. Provided with food by his tribesmen, enjoying a monopoly of trade in highly prized products, entitled to a share in doctors' fees, he was the wealthiest man in the community. By way of reciprocity, it was his duty to entertain visitors, to help the poor, and to contribute generously to the cost of festivities. He determined movements from and to the village and alone could authorize the death penalty for a public enemy. In general, he adhered to the part of a peace-preserving headman, rarely making a vital decision without previously consulting other venerable men.

Nevertheless, a chief could *de facto* magnify his power with the aid of a favourite shaman. In lieu of taxation the Yokuts expected the persons attending a festival to defray the expenses. If a wealthy villager evaded this obligation, the chief's medicine-man would smite him with illness and impoverish his victim by exorbitant fees for sham treatment. Since the chief's consent was essential for violent measures against the doctor, he could always dismiss complaints on the subterfuge of insufficient evidence. It is important to note that public opinion as a rule

³⁸ A. H. Gayton, *Yokuts-Mono Chiefs and Shamans*, Univ. Calif. Publ. Amer. Arch. and Ethn., XXIV (1930).

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sympathized with the chief and the shaman, for the miser who failed to contribute at festivals thereby imposed extra burdens on his fellows.

Given the native faith, an unscrupulous chief could evidently work his will in collusion with a shamanistic accomplice. Yet in the long run, Dr. Gayton explains, such knavish tricks led to a revulsion of feeling. A chief could not safely give rein to his malevolent inclinations. In the face of continuous suspicion his prestige would wane, in extreme instances he might even be supplanted in office by a less objectionable scion of his line. As for his accessory, the attitude toward doctors being ambivalent here, as in much of North America, a persistently malevolent leech was likely to be killed by the enraged family of his victim. In short, the Yokuts system involved a considerable strengthening of chiefly influence without, however, approaching anything like despotic rule. Its instructiveness lies largely in demonstrating religion as a prop of the civil head on the relatively low plane of a simple hunting people.

In a not inconsiderable number of South America societies there is a personal union of temporal and spiritual functions. In Colombia, the Kágaba and the Ijca (the latter linguistic relatives of the Chibcha) do not dissociate the concepts of priest and chief. Among the Yaruro (Venezuela) each moiety recognizes a shaman as its head. In the Mato Grosso the Tupi-Kawahib chief is "first of all, a shaman, usually a psychotic addicted to dreams, visions, trances and impersonations." Another Brazilian group, the Botocudo, had as the leader of a band the "strongest" man, the epithet designating not muscular strength, but spiritual ascendancy. And, suggestively enough, these chiefs played a greater role than their colleagues in neighbouring populations and were in higher measure responsible for their bands, which sometimes took their names from the leaders'.³⁹

But even the Botocudo chief's influence pales before that of

³⁹ Gustaf Bolinder, *Die Indianer der tropischen Schneegebiete* (Stuttgart, 1925), pp. 111 ff., 126 ff.; K. T. Preuss, "Forschungsreise zu den Kagaba-Indianern der Sierra Nevada de Santa Marta in Kolumbien," *Anthropos*, XIX-XV (1919-1920), 364-368; Vincenzo Petruccio, *The Yaruros of the Capanaparo River*, BAE, Bull. 123 (1939), p. 215; Nimuendajá, "Social Organization and Beliefs of the Botocudo of Eastern Brazil," pp. 97 ff.; Lévi-Strauss, *op. cit.*, p. 25.

the prophets who periodically arose in both Americas.⁴⁰ In my opinion, Nimuendajú, Spier, and Métraux have demonstrated that these messiahs did not so much react against white aggression, which represented merely a special case of the generic problem of evil, as against the supposed doom that threatened to engulf the moribund universe. Typically, the prophets promised salvation to their adherents, whereas unbelievers were to be transformed or destroyed. Given the mental atmosphere of the aborigines, the more dynamic of the messiahs undoubtedly gained an extraordinary sway over their fellows. One of the early Guarani deliverers affected the pomp of royalty: refusing to walk, he had himself carried on the shoulders of his attendants; the common herd were not allowed to approach his person. Such pretensions rested on a claim to supernatural inspiration or to divinity itself. Many of the self-styled saviours tyrannically imposed their will against common sense and, what is far more, against previously entrenched beliefs. In order to dance and chant as required, Obara's Guarani followers ceased to plant and harvest their crops in 1579. In the nineteenth century, under the spell of successive prophets, the Apapocuva band of this people repeatedly chased the will-o'-the-wisp of an earthly paradise, undertaking lengthy migrations to escape the menacing catastrophe. A little over a century ago an Algonkian messiah successfully ordered his people to kill their dogs and to abandon their hitherto prized sacred bags. For a while, about 1805, Tenskawatawa, the Shawnee prophet, even held the power over the lives of his tribesmen, having his opponents burnt as witches.

It is a far cry from the unstable sovereignty of these prophets to the close-knit Inca state, but the gap is far greater between the nominal chiefs described by Tanner among the Central Algonkians and the messiah he met in the very same tribe. The former were obeyed when the people so chose; at the latter's

⁴⁰ Mooney, *op. cit.*, pp. 662, 672 ff., 676, 686, 700; Leslie Spier, *The Prophet Dance of the Northwest and Its Derivatives: The Source of the Ghost Dance*, General Series in Anthropology, No. 1 (Menasha, Wis., 1935); A. Métraux, "Les Hommes-dieux chez les Chiriguano et dans l'Amérique du Sud," *Revista del Instituto de Etnología de la Universidad Nacional de Tucumán*, II (1931), 61-91; Curt Nimuendajú, "Die Sagen von der Erschaffung und Grundlagen der Religion der Apapocuva-Guarani," *Zeitschrift für Ethnologie*, XLVI (1914), 284-403.

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behest they humbly killed their dogs, gave up their strike-a-lights at the expense of "much inconvenience and suffering," and threw away their hitherto holiest possessions. Assume the urge to leadership, as found by Lévi-Strauss in the Mato Grosso, to be combined with an awe-inspiring supernatural sanction, and the way is clear to a formative stage on the way toward a government by divine right. What military prowess failed to create in aboriginal America is demonstrably possible even in a democratic environment under the hypnosis of religious exaltation and the moral duress that follows in its wake.

CONCLUSION

It is not part of my plan to squeeze out of the evidence conclusions it will not bear. I cannot trace in detail the sequence of events that led from Ona "anarchy" to the close-knit structure known as the Inca state. I rest content with sketching a probable line of development. The totalitarian concentration of power in Inca Peru is an historic fact; so is the absence of any comparable official authority over most of the New World. If, for the sake of throwing the problem into relief, we assume an otherwise unwarranted teleological point of view, we discover sundry gropings towards the establishment of political authority, which, however, lose themselves in blind alleys. On analogy, what seems simpler than a military despotism under the two Iroquois generals? Yet nothing of the sort arose in the face of an antagonistic cultural tradition. Similarly, the workings of the Cheyenne military societies seem to predestine the tribe to an oligarchical system; but that, too, was precluded by the regnant pattern of social life.

Nevertheless, equalitarianism recedes when confronted with putative supernatural favour. The very same men who flout the pretensions of a fellow-brave grovel before a darling of the gods, render him "implicit obedience and respect." It is probably no mere coincidence that Pontiac was a higher priest in the most sacred organization of his people, that Tecumseh was seconded by his brother, the prophet, and on occasion himself laid claim to supernatural powers. The foundation of a major state, I suggest, was due to men of this type—men who both

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imagined a unity beyond that of immediate kinship and contiguity and who simultaneously succeeded in investing their mission with the halo of supernaturalism. When not pitted against the terrible odds actually encountered by Pontiac and Tecumseh, natives of their mentality would be able to overcome both the dominant separatism and the dominant libertarianism of their fellows and create the semblance of a modern state.

PART IV *Theories and Theorists*

Characteristic of Lowie's writings on theoretical subjects is his appraisal of the work of particular men. To this approach his outstanding book *The History of Ethnological Theory* (1937) is ample testimony. It may interest the reader to compare the chapter on Tylor in that book with the article on him which Lowie wrote as an obituary for the *American Anthropologist* in 1917 (No. 25 in the present volume), or to compare the chapter on Lewis H. Morgan with "Lewis H. Morgan in Historical Perspective" (No. 26), written at almost the same time (1936) for A. L. Kroeber's *Festschrift*. Boas was so recurrent a preoccupation for Lowie that at least one of not less than seven papers on his great teacher should be included here (No. 29). Laufer's careful historical studies were much admired by Lowie and his students were grounded in the work of that distinguished sinologue. Although Laufer is less in fashion these days, an unpublished appreciation of him is included here (No. 32), as I believe Lowie would have wished.

"A New Conception of Totemism" (No. 22) and "On the Principle of Convergence in Ethnology" (No. 23) are two of Lowie's early classics, and no collection of his writings would be complete without them. The first was stimulated by his attentive reading of Goldenweiser and the second by his equally thoughtful reading of Graebner.

Lowie was by no means a cultural antievolutionist, as a careful

reading of even this incomplete series of papers will reveal. His interpretation of what constitutes cultural evolution, however, warrants consideration. Therefore "Evolution in Cultural Anthropology: A Reply to Leslie White" (No. 28) has been included here, although I have on the whole avoided reproducing his more controversial articles as well as his more readily available ones.

The two parallel articles, "Cultural Anthropology: A Science" (No. 27) and the hitherto unpublished "The Development of Ethnography as a Science" (No. 33) serve to illuminate Lowie's strongly rational and empiric convictions concerning science. So also does the article "Some Problems of Geographical Distribution" (No. 30). The reader may wish to supplement the articles with "Ethnography, Cultural and Social Anthropology" (*American Anthropologist*, LV [1953], 527-534). Finally, "Contemporary Trends in American Cultural Anthropology" (No. 31), which Lowie wrote two years before his death, is a characteristically fair, informed, and objective appraisal of the field to which he devoted his life.

A New Conception of Totemism

THE SIGNIFICANCE OF DR. GOLDENWEISER'S RECENT PAPER on totemism¹ lies in the fact that it presents for the first time what may be legitimately called "an American view of totemism,"—"American" not only because it takes into account the data of American ethnography, but in the far more important sense that it is a view based on methodological principles which are becoming the common property of all the active younger American students of ethnology.

According to the traditional view, totemism is an integral phenomenon which is everywhere essentially alike. Thus, in Frazer's latest work on the subject, *Totemism and Exogamy*, the burden of proof is explicitly thrust on the shoulders of those who question the identity of totemic phenomena in different quarters of the globe and who uphold the theory of convergent evolution. In Part I of his paper "Australia and British Columbia," Dr. Goldenweiser has anticipated this challenge. He selects the series of features that are commonly regarded as distinctive of totemism, and compares the forms they assume in the two areas

American Anthropologist, XIII (April-June, 1911), 189-207

¹ A. A. Goldenweiser, *Totemism, an Analytical Study*, reprinted from *Journal of American Folk-Lore*, XXIII (April-June, 1910), 115 ff.

considered. The result is sufficiently striking. On superficial consideration, it appears that the Australian totem group resembles the clan of British Columbia in the exogamic regulation of marriage. But this resemblance is not significant; in both cases the exogamous character of the clan is not a primary, but a derivative trait. Because the clans are, in both areas, parts of the larger phratric units, and because these phratries are exogamous, the totem clans must be exogamous, even though the clan, as a clan, may have nothing to do with exogamy. In other features, the totem clans of Australia and British Columbia clearly diverge. In Australia the social importance of the clan dwindles into insignificance as compared with that of the phratry; in British Columbia the clan is the social unit, *par excellence*. On the Northwest coast there is evidence for the development of the clans from village communities, such as nowhere exist in Australia. Finally, the American clans are graded as to rank—a condition likewise lacking in Australian totemism. In the matter of clan names, what similarity exists is again of a superficial kind. In Australia all clans are named from their totems; in British Columbia clans frequently derive their names from localities. But precisely where the American social divisions (phratries) *are* named after animals, we occasionally find that the eponymous animal is not identical with the crest animal, which is the one that corresponds, in religious function, to the Australian totem. If phratries are compared it is found that those of the Tlingit and Haida bear animal names, but that only a few of the Australian phratry names are definitely known to refer to animals. The view that the totemite is a lineal descendant of his totem is clearly developed in Australia; on the Northwest Coast, on the other hand, there is a fundamental belief in *human* descent: the crest animal is one which has in some way been associated with the human ancestor of the group. Nevertheless, the author points out, there are myths in which the association is very close, and, in one group of traditions, the ancestor is the crest animal transformed. These instances, instead of militating against the author's point of view, constitute in reality strong evidence in support of it. For the myths in question result from the reaction of the guardian-spirit concept upon the basic belief that human

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beings have human ancestors. Now, the guardian-spirit concept is practically foreign to Australia. What similarity there exists between the Australian and the American myths is accordingly an ideal instance of convergent evolution. There remains the criterion of the taboo against eating or killing totems. Of this phase of totemic life Australia remains the classical example; in British Columbia, on the other hand, not a single instance of totemic taboos has hitherto been discovered, though there is an abundance of taboos of non-totemic character.

A survey of the currently assumed symptoms of totemism in the two areas discussed thus reveals far-reaching differences. It would be artificial, however, to confine the comparison within the limits set by conventional definitions of totemism. If we wish to disabuse ourselves of the preconceptions expressed in these definitions, Dr. Goldenweiser insists, we must not neglect to consider those cultural features which are empirically found in intimate association with the criteria generally recognized as totemic. In Australia, two elements have risen to so commanding a position within the totemic complex that each has been assumed as the essence and starting-point of totemism generically. These elements are the *intichiuma* ceremonies conducted for the multiplication of the totem animals, and the belief in the re-incarnation of ancestral spirits. On the Northwest Coast of America, analogous features are indeed found, but they are wholly dissociated from totemic institutions. A parallel condition of affairs is revealed in viewing the dominant traits of social life in northwestern America. The social life of the Kwakiutl is unintelligible without taking into account the groups of individuals sharing the same guardian spirit; among the tribes farther north the clan tradition is essentially an account of the ancestor's acquisition of his guardian spirit, while the circumstances incident thereto are dramatized in the dances of the secret societies. In Australia guardian spirits are rare, and, where found, are generally quite distinct from the totems; even when the two concepts do coincide, the guardian-spirit factor is of relatively slight moment. A second trait of special significance in the American area is the relationship of totemism to art,—the saturation of practically all decorative attempts with totemic

motives, and the retroactive tendency to give, secondarily, a totemic interpretation to designs purely decorative in origin. This intimate connection is largely dependent on the quasi-realistic style characteristic of Northwest American art. In Australia, where geometrical motives predominate, art has exerted but little influence on totemic life.

The empirical consideration of the totemic complex in the two typical regions dealt with thus establishes the essential diversity of the phenomena compared. The dominant motives of Australian totemism are not the dominant motives of Northwest American totemism, and vice versa. What resemblances exist are either superficial, or are functions of traits not directly associated with totemism. Here, however, the criticism might be made that totemism in the areas selected is not comparable because the American institution represents a far later stage of development. "The totem," as the author puts it in anticipation of this stricture, "has become attenuated to a crest, to a symbol; the living, flesh and blood relationship with the totem animal has been transferred into the realm of mythology; and, naturally enough, the taboo on the totem animal has dwindled away and finally disappeared." Dr. Goldenweiser's answer is unmistakably clear. "To a retort of that character, I would answer that we may safely assert that there is not one phase of human culture, so far represented in an evolutionary series of successive stages of development, where the succession given has been so amply justified by observation of historic fact as to be safely adopted as a principle of interpretation" (p. 22). The evidence from Northwest America must thus be admitted as coördinate with that from Australia.

Having demonstrated the validity of the theory of convergent evolution for the totemic phenomena of Australia and the North Pacific Coast of America, the author proceeds in the second part of his paper, "The Totemic Complex," to consider, one by one, the traditional elements of totemism, and to determine their mutual relations on the basis of the widest possible ethnographic basis.

In the first section of Part II, Dr. Goldenweiser takes the important step of divorcing exogamy from the concept of totemism.

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A discussion of the data on the natives of Assam, the Nandi of East Africa, several Plains Indian tribes, and the Samoan-Fijian culture province leads to the conclusion that "clan exogamy, although a usual concomitant of the other totemic features, is not a constant, hence not a necessary, concomitant of the latter; and again, where the other features are absent, exogamy may nevertheless occur" (p. 55).

The pages immediately following embody what is probably the author's most important and original contribution to the subject,—a critique of the concept of exogamy itself. Not only may clans exist independently of exogamic rules, and exogamic rules independently of clans, but even where clan exogamy does exist the union of exogamy with the clan unit may be a secondary feature. In the case of the Kurnai, to be sure, special conditions seem to warrant Howitt's conclusions that marriage was originally regulated by non-localized totem clans, that paternal descent effected a localization of the totems, and that consequently, in recent times, locality has appeared as the regulating factor. But we should not by any means be justified in transferring this mode of reasoning to other areas where different conditions prevail. In British Columbia, for example, it seems probable that the clans at one time occupied separate villages. Now, whether the clans were exogamous as clans, or because they happened to coincide with local exogamic divisions, becomes an open question as soon as the existence of exogamy dissociated from clans has been established. The point comes out even more clearly where kinship exerts an influence on matrimonial regulations. Among the Todas a purely objective investigation reveals a number of exogamous clans, as well as a series of matrimonial rules based on kinship. But, subjectively considered, the matter is quite different. The Toda merely knows that certain relatives through the father and certain relatives through the mother—all of them included in the term *pūliol*—are not marriageable persons for him. Besides members of other clans, an individual's *pūliol* group also embraces *all* the people of his own clan, but this fact does not seem to have been noticed by Rivers' informants before he pointed it out to them. Similarly, among the Blackfoot, members of a band are forbidden to intermarry, not

as band members, but because they are considered blood relatives. In a manuscript by Sternberg, which the author lays under contribution for additional facts, the Gilyak are described as a people with exogamous gentes. Now, where gentes as such are the exogamous units, two gentes suffice for the regulation of tribal marriages, the men of gens A marrying women of gens B, and vice versa. This reciprocal relationship is precisely what does not obtain in Gilyak society. If the men of gens A marry women of gens B, the men of gens B are *ipso facto* debarred from marrying women of gens A, the two gentes being regarded as gentes of "sons-in-law" and "fathers-in-law," respectively. The men of B must thus marry women of gens C, and so forth. It is, accordingly, obvious that the gens as such does not determine marriage.

The thesis is thus established that "when the fact of a given social group not marrying within itself is ascertained, the information acquired is but partially complete" (p. 59). The exogamous nature of a group, as objectively observed, may indeed be a primary trait; but it may also be a derivative trait,—a necessary consequence of other regulations not linked with the group as such. Dr. Goldenweiser is thus emboldened to inquire, whether the exogamic character of Australian totem clans is a primary or a secondary characteristic. Taking up first the simpler form of social organization typified by the Dieri, viz., two exogamous phratries subdivided into smaller totem groups, he finds that a given clan can not be considered an exogamic unit because in no case are the exogamic marriage regulations fully determined by *clan* affiliation. If this condition *did* obtain, nothing would prevent members of clan *a* from marrying members of clan *b* of the same phratry. But this is emphatically not what takes place. In reality, clan *a* "behaves exactly as would an individual of phratry A if there were no clans" (p. 60). Because it forms part of a larger unit exogamic in its own right, clan *a* must *ipso facto* be exogamic. The condition of affairs is strictly parallel to that among the Tlingit and Haida Indians. In Australia the derivative character of clan exogamy is illustrated among the anomalous Aranda (Arunta), where some clans occur in both phratries. Here a man of clan *a* may marry

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a woman of clan *a* if she belongs to a different phratry, but he must not marry her if she belongs to the same phratry.

The argument is greatly strengthened by a consideration of the four-class system. For here the *class* is the marriage-regulating unit, and the clans are in no sense exogamous units, as each clan contains two sets of members with distinct matrimonial regulations. In tribes with the eight-sub-class system a parallel argument holds: the sub-class is the marriage-regulating unit, and each clan consists of "four matrimonially heterogeneous units." The question arises, what, in these systems, may be the marriage-regulating functions of the phratry, and of the phratry and class, respectively? Dr. Goldenweiser is of opinion that in the four-class system the fact beyond doubt is the exogamy of the *class*, while that of the phratry remains to be investigated; in the eight-sub-class system the immediate data indicate the exogamous nature of the *sub-class*, and the matrimonial functions of the class and phratry remain to be investigated. The point to be determined would be the native feeling with regard to these larger units,—whether, for example, the phratry of four-class tribes continues in the minds of the natives to constitute a distinct exogamic group. The necessity of taking into account the subjective attitude of the natives is strikingly illustrated in perhaps the most suggestive passage of the entire paper. A purely objective description of the regulations found among four-class tribes does not by any means necessitate the current mode of representation. Instead of subdividing two phratries into two exogamous classes, it is possible to unite the intermarrying classes into *endogamous* moieties with exogamous subdivisions. This has actually been done by Professor Klaatsch, a relatively naïve observer in matters ethnological, in the description of Niol-Niol social organization. This traveler has even recorded native names for the endogamous moieties. Dr. Goldenweiser rightly insists that, objectively, Klaatsch's mode of representation is as legitimate as the one ordinarily employed. His suspicions are aroused merely by the fact that endogamous moieties *recognized as such by the aborigines* have hitherto escaped the eyes of other Australian ethnographers. Moreover, the class names of the Niol-Niol apparently correspond to those of the Aranda, whose phratry-

class organization seems firmly established. Accordingly, the author does not contend that Klaatsch's scheme represents the subjective facts, though he admits that "on a par with the dominant phratric organization there may also exist in these Australian tribes a consciousness of the objectively endogamous groups constituted by the pairs of intermarrying classes" (p. 64).

In this extraordinarily illuminating discussion the critic can find fault only with the author's use of the term "exogamy." A word is obviously required to designate the rule against members of a group marrying among themselves,—in other words, the rule of the incest group. The word sanctioned by usage is the etymologically unexceptionable and self-explanatory term "exogamy." Dr. Goldenweiser, however, modeling his conception of the "typical exogamous relation" on the conditions supposedly found in Australian tribes with two phratries, writes: "An exogamous relation is fully represented only when both the group within which marriage is prohibited, and the one into which it is permitted or prescribed, are given" (p. 60). Accordingly, he views the class (among four-class tribes) as the exogamous unit par excellence, and finds an approach to "pure totemic exogamy" in the Arábana institution of each totem clan being permitted to intermarry with only one particular clan of the complementary phratry. Were the matter one purely of nomenclature, the redefinition of a current term would, of course, be perfectly legitimate. In the present instance, however, it seems to the critic that the term is not, and can not conveniently be, used with consistency in the modified sense. Where there are only two social units exogamous in their own right, intermarriage follows as a physical necessity; the group into which marriage is permitted or prescribed is determined by the mere statement of the prohibitory regulations. This is obviously not the case when there are four, or six, or fourteen groups, within each of which marriage is prohibited. To be sure, it might be said that in such instances the exogamous relation, in Dr. Goldenweiser's sense, is fully represented, inasmuch as, where statements to the contrary are lacking, a member of group I may marry members of all other groups. But if positive regulations are to be taken into account, it certainly is not the same thing whether a man *must*

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marry into the only group existing besides his own, whether he must marry into *one* of a number of other groups, or whether he may marry into *any* of the other groups extant. To the critic it seems that there are only two alternatives. Either we adopt the author's conception of exogamy. Then the mutual relationship of intermarrying classes with rules against intra-class marriage would form the standard illustration of exogamy; phratries would formally, but, for reasons just given, might *only* formally, exemplify exogamy; and it would be inadmissible to speak glibly of four exogamous Tsimshian clans (p. 9), of a great number of exogamous Khasi clans (p. 53), of fourteen exogamous Bahima clans and forty-one exogamous septs (p. 74). Or, we cling to the accepted usage of the term. Then exogamy may be ascribed to any group prohibiting marriage among its members. In this case, the exogamy of the Kamilaroi class, as well as the exogamy of the Arábana clan, is a derivative feature, —a logical consequence of phratric exogamy. In addition to this derivatively (and therefore relatively unimportant) exogamic trait, the Kamilaroi class and the Arábana clan have certain positive marriage-regulating functions, which, however, have nothing to do with exogamy, of which the functions are only prohibitory.

In the next part of the section on "Exogamy and Endogamy," the author briefly mentions the constant tendency to extend regulations of marriage, even where fairly definite regulations already exist. An unusually suggestive instance is furnished by the Toda (p. 168). Within the (endogamous) Teivaliol moiety there are a number of exogamous clans. But the members of the Kundr clan outnumber the other clans to such an extent that the exogamous rule can only be followed by the Kundr marrying most of the members of the other clans. Thus very few of the latter are left to marry one another, and the condition of affairs seems to approach as a limit the widespread division of a tribe into two exogamous intermarrying phratries. The occurrence of *positive* obligations for certain classes to intermarry —a point too little noticed by other writers—is strikingly illustrated by the Gilyak groups of prospective husbands and wives. Dr. Goldenweiser, in discussing the matrimonial institutions of

this people, also calls attention to the correlated rules of "*psychic intercourse*." There is restriction of conversation and intimacy between persons who might come into conflict from jealousy, and avoidance obtains, in different degrees of stringency, between relatives debarred from intermarrying. On the other hand, there is great freedom between prospective husbands and wives, and an extraordinary cordiality characterizes the relations of fathers-in-law and sons-in-law. There can be little doubt that the correlation of the rules for sexual and psychic intercourse, which the author considers only in connection with a single tribe, merits more extensive investigation.

The next two sections, "Totemic Names" and "Descent from the Totem," add little to the argument of the corresponding divisions of Part I. Some additional examples are adduced to show that eponymous totems, while remarkably frequent, do not occur universally, and that the totem is not invariably regarded as the ancestor of the group. Under the heading "Taboo," the author points out that quite generally restrictions of conduct are associated with groups other than totem clans, while there are tribes, such as those of the Iroquois confederacy, whose totem clans are not connected with taboos against killing or eating the eponymous animal. In reply to the specious reasoning of many writers, that totemites abstain from killing or eating their totems *because* they regard them as kin, Dr. Goldenweiser intimates that taboos may have a variety of origins. The Omaha furnish a telling series of illustrations. For here many of the taboos associated with totem groups are logically unconnected with the totems, and it seems practically certain that each of these "fanciful prohibitions" had a distinct origin.

In the pages on "The Religious Aspect of Totemism," the author emphasizes the fact that totemism and animal cult are distinct phenomena. Animal worship is prominent where totemic groups do not exist. On the other hand, worship of the totem is very rare, and in some cases there is a complete absence of religious associations with totems. It is obvious that under the circumstances it would be absurd to regard totemism as a form of religion, or as a distinct stage in the evolution of religious beliefs. On this point, at least, Dr. Goldenweiser finds himself

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in agreement with the views put forward in Frazer's most recent publication on the subject.

The comprehensive survey of ethnological phenomena in Part II thus confirms the conclusions arrived at from a comparison of Australian and British Columbian conditions. Totemism can no longer be considered as an integral phenomenon. Totemic complexes are "conglomerates of essentially independent features" (p. 88). It may be possible to trace logically the development of the several traits from a single hypothetical factor of fundamental importance, but only through historical proofs can such deductions gain scientific value even for limited areas. Neither a system of naming groups after totem animals, nor the doctrine of descent from the totems, nor a religious regard for the totem, in fact, not one of the symptoms ordinarily assumed, is a constant feature of totemism; and there is no evidence for the historical or psychological primacy of any one of them. The instances of other factors—magical ceremonies in Australia, esthetic motives in British Columbia—rising to prominence within the totemic complex illustrate the variability of the phenomenon studied, and lead to the important queries, "If totemism includes, roughly speaking, everything, is totemism itself anything in particular? Is there anything specific in this phenomenon, or has the name 'totemism' simply been applied to one set of features here, to another set there, and still elsewhere perhaps to both sets combined?" (p. 89).

Dr. Goldenweiser replies that, in the light of his foregoing analysis, the specific trait of totemism cannot be a certain definite sum of elements, but only *the relation obtaining between the elements* (p. 92). In a given totemic complex, factors *a*, *b*, *c*, etc., are associated and correlated so as to form a relatively integral combination. The fairly complete integration of totemic factors results from the fact that elements in themselves socially indifferent become associated with clearly defined social groups, the association being effected by means of descent (p. 93). In defining the relationship of the totemic elements, the author starts from a consideration of the current view that totemism has a religious and a social aspect. The occasional absence of any religious factor, notably among the Iroquois, induces him to

eliminate the term "religious" and to conceive totemism as the association of "objects and symbols of emotional value" with definite social units, the latter being defined as units perpetuated through descent. Again, totemism is usually described as a static phenomenon. Yet, nothing is more obvious than its variability in time. Dr. Goldenweiser's investigation, accordingly, culminates in the dynamic definition: "Totemism is the process of specific socialization of objects and symbols of emotional value" (p. 97).

As an epilogue, the substance of which would have more appropriately preceded the definition of totemism, comes a discussion of "Origins, in Theory and History." Schmidt's, Frazer's, and Lang's theories are jointly subjected to a methodological critique. Instead of attempting to understand present conditions on the basis of their established antecedents, these theories select a prominent feature of modern totemism and project it into the past, assuming it to be the starting-point of the totemic process. This, the author contends, is unjustifiable; for what is *now* of overshadowing significance need not always have figured with equal conspicuousness. The second step made by the theorists mentioned, namely, the deduction of other features from the one assumed to be primary, is likewise illegitimate; for it assumes the unity of the totemic features and a uniform law of development. The former assumption has been refuted by the preceding analysis, while the latter seems doubtful in the light of modern research. Finally, the authors criticized err in neglecting the influence of borrowing on the development of culture in a given area.

To bring home this last point, Dr. Goldenweiser proceeds to show what the course of totemic development has actually been in the carefully studied region of British Columbia. While the southern Shuswap have the loose village organization typical of the Salish tribes of the interior, the western Shuswap have a social system obviously patterned on that of the coastal tribes, and indirectly derived from them. Among other instances within the same area, the transformation of the institutions of certain Athapascan tribes is especially remarkable. Such features as potlatches, clan exogamy, and an hereditary nobility, have been obviously borrowed from neighboring coastal tribes; and

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in so far as the Athapascan tribes possessing these traits differ in the details of these institutions, the differences can sometimes be directly explained by contact with correspondingly differing tribes of the coast. The actual history of such changes could never be foretold by means of speculations as to primitive psychology; it was ascertained only by intensive study of the influences to which each tribe has been subjected (p. 109). In the data already accumulated on Australia, Dr. Goldenweiser finds evidence of the far-reaching influence of diffusion on cultural development; and his paper terminates in the confident prophecy that future research will reveal conditions of borrowing comparable to those established in British Columbia. As English ethnologists seem to adopt only with reluctance the historical point of view advocated by other students, it may be well to recall Tylor's memorable words:

Most of its phenomena (that is, of human culture) have grown into shape out of such a complication of events, that the laborious piecing together of their previous history is the only safe way of studying them. It is easy to see how far a theologian or a lawyer would go wrong who should throw history aside, and attempt to explain, on abstract principles, the existence of the Protestant Church or the Code Napoléon. A Romanesque or an Early English cathedral is not to be studied as though all that the architect had to do was to take stone and mortar and set up a building for a given purpose (*Researches into the Early History of Mankind*, p. 4).

The historical significance of Dr. Goldenweiser's essay will perhaps become clearer from a parallel between the development of ethnological thinking and the evolution of philosophical thought in general. Popular philosophy has always had the tendency to assume a necessary bond between the constituents of a relatively stable complex of observed elements,—to assume that there is a "thing" which *has* properties, an *ego* which *has* sensations, feelings, and other manifestations of consciousness. Valuable as such summaries of experience are from a practical point of view, they become indefensible from a higher standpoint. The ideas we form of "things" result from an association (by contiguity) of the ideas of its properties. A child learns by experience that a brown patch of color and a certain form of

resistance to the touch are linked together, and by connections of these ideas develops the idea of a table. A "thing" is thus nothing distinct from its properties; it is nothing but the sum-total of these properties; there is no mystic unity in reality apart from the properties. (Cf. Höffding, *Psychologie*, pp. 212, 226, 285.) The ethnologist, like the uncritical philosopher, is confronted at every step with conjunctions of features which at first seem indissolubly united. A geometrical pattern is associated in the primitive craftsman's mind with some definite animal or plant. It is natural to assume that the association is a primary one,—that the design is a degenerate attempt at realistic representation. Games are played as means of divination or processes of sympathetic magic. Should they not be conceived as ceremonial contrivances? Tales of heroic exploits culminate in the hero's ascension to the sky. Must not the whole plot be a function of his celestial affiliations? Social units with animal names and food taboos prohibit marriage within the group. To regard names, taboos, and exogamous rules as merely manifestations of the same fundamental phenomenon is, at a relatively early stage of inquiry, the obvious and psychologically most intelligible thing to do.

At a more critical stage, however, the instability of the complexes attracts notice. What was at first supposed to be a necessary connection is reduced to a mere conjunction of elements. Thought is no longer arrested by a contemplation of the mystic underlying units and their relations with the observed elements; to determine the nature and interrelations of these elements themselves becomes the highest, nay only possible, goal of investigation. In the domain of physical science, a critical reformation of this type has been, within recent decades, effected by Professor Ernst Mach. In ethnology, the school which has set itself a corresponding aim, which endeavors to supplant the traditional belief in mystic ethnological complexes with a deeper, though, it may be, still only proximate, analysis into provisional elements, is the school headed by Professor Franz Boas. Under his influence Kroeber and Wissler have shown that the same pattern is subject to varying interpretations even within the same tribe: design and interpretation are found to correspond

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to distinct psychological processes. An analogous conclusion with regard to the conjunction of story plot and cosmic phenomena has been drawn by the present writer. Independently of Boas, but in thorough harmony with his point of view, Seler, in criticizing Preuss, and Haddon, in criticizing Culin, have pointed out *en passant* that the association of ritual with forms of diversion is a secondary development. What all these writers have attempted in the study of their own problems Dr. Goldenweiser has done for the far more complicated subject of totemism. He has shown the futility of attempting to connect any definite conception of concrete ethnological facts with the term "totemism." He has shown that there is no justification for assuming a common substratum underlying all the "totemic" complexes: a complete statement of all the social, religious, esthetic, and other correlates with their interrelations, as found in a given area, exhausts the possibilities of description and explanation.

However, as already shown in the résumé of the section on "The Complex in the Making," Dr. Goldenweiser does not abandon the term "totemism," but seeks to justify its retention by a redefinition of the word from a dynamic standpoint. It is here that he passes beyond the limits reached by his fellow-students of secondary associations. For, while the latter are generally content to indicate the fact that a secondary association of elements has occurred, Dr. Goldenweiser boldly undertakes to define, with some precision, the process itself of the association. That is to say, he does not merely hold that totemism is the result of a secondary association of social units with various factors. He holds, in addition, that the association resulted from the fact that objects and symbols which were originally of emotional value only to individuals became, through descent, values for definite social groups (p. 97).

Before entering into a critique of this conception, it is worth noting that many forms of association not ordinarily considered totemic would be classed as such according to the new definition. A phratry and a local group might illustrate the dynamic process in question as well as any "totem kin" of other writers. In particular, the fact that the *name* occupies no favored position, but appears as but one factor of many that may be associated, seems

to render "totemism" almost all-inclusive. This is especially the case when we consider that, on the author's theory, it is not at all necessary that the names be derived from animals or plants. Discussing Iroquois totemism (p. 96, footnote), Dr. Goldenweiser argues that even here, where the totem is merely a name, it, at least formerly, represented an emotional value, inasmuch as otherwise the name would not have become firmly fixed in social groups. Obviously, the same reasoning—which the reviewer cannot consider conclusive—would apply to local units with non-animal names. It is not clear whether, or where, the author would draw the line here; indeed, the data bearing on names of totem groups require more extensive treatment than that given in the present paper before it will be possible to form a clear view of Dr. Goldenweiser's conception of this special point.

Dr. Goldenweiser's definition of totemism may be considered from two points of view. In how far does it accurately represent the phenomena commonly designated as totemic? And, to what extent does it represent the totality of phenomena which seem psychologically and sociologically related with these totemic phenomena?

In reply to the first query, it must be admitted that the author's definition outlines a plausible course of development. Nevertheless, it is possible to conceive that conditions other than those defined by Dr. Goldenweiser may lead to typical totemism. Assume two locally distinct groups, each with its own taboos against the eating of a certain animal. Then the union of these two groups would lead to a typical totemic society, in the ordinary sense of the term, if we add the feature of exogamy. Such a hypothetical development in no way militates against the author's general point of view. Nevertheless, it is perfectly easy to understand the process, from what we know of the development of taboos, without recourse to the theory that the taboo was originally of only individual significance and afterwards became socialized through descent. Or, to take a case which is not hypothetical. What evidence is there to show that among the Iroquois the clan name was originally an individual possession which, through descent, became socialized? To exclude instances of this type from the list of totemic phenomena by a

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rigorous application of the definition would reduce the whole discussion to a logomachy, which would be entirely beside the author's purpose. For what he attempts to do is precisely to define the essential features of the process resulting in what are ordinarily called totemic phenomena. The fundamental objection to such a definition as Dr. Goldenweiser has attempted is that it is frequently impossible to determine whether it correctly represents the historical process of association. If we assume the association of name and social group as the starting-point of totemism—and, as the author himself has shown, this combination sometimes exhausts the content of totemism—it is, in our ignorance of the actual history of the development, impossible either to prove or to refute the theory that the group names, not only in the Iroquois, but in the Australian cases as well, ever served to designate individuals. The inherent probability of such a condition does not seem very great. If the association of *taboo* and social group is taken as the starting point, the a priori probability of a socializing process will presumably appear considerably greater to the majority of ethnologists. Nevertheless, the hypothetical instance given above seems to indicate that socialization is not a *Denknotwendigkeit* for the comprehension of the established association. The critic is therefore of opinion that a non-committal attitude on the process of association (so far as it eludes observation) is highly advisable. Totemism would then be defined, not as a *socialization* of various elements of (at least potentially) emotional value, but merely as the *association* of such elements with social groups.

The second question is, does Dr. Goldenweiser's conception embrace all the phenomena essentially related to those of totemic phenomena generally recognized as such? The writer feels that, inclusive as is Dr. Goldenweiser's definition, it limits the field of totemism too narrowly by an exaggerated emphasis of the element of descent. By a "complete social unit" Dr. Goldenweiser understands one group of at least two within the tribe, each including both men and women, and perpetuated by descent (pp. 93, 94, 97, 98). Accordingly, in dealing with the resemblance between totemic institutions and religious societies whose members share the same guardian spirit, he does not discover a genuine

homology. "While a certain psychological affinity between the two institutions is not improbable, their genetic relationship, claimed by some, calls for demonstration" (p. 94). The matter of genetic relationship may be dismissed at once as irrelevant, for as Dr. Goldenweiser, on the very next page, states his belief in the convergent evolution of totemic phenomena, absence of genetic connections would not, from his point of view, bar religious organizations from the fold of totemic institutions. Their exclusion, then, rests essentially on the definition of a social unit. Now, the definition given by Dr. Goldenweiser seems to the writer quite arbitrary. If the peculiarity of totemic phenomena lies only in the relation obtaining between the elements (p. 92), the psychological resemblance of this relationship would seem to be the predominant issue, while the precise nature of the social group becomes negligible. Among the Gros Ventre (Atsina), where every man passes successively through a series of age-societies, these grades are well-defined social units. The association with each of them of a certain animal for which several of the societies are named does not seem to differ in principle from the association of a clan with its crest or eponymous animal ancestor. It may not be out of place here to refer to the fact that Schurtz has already darkly hinted at a connection between totemism and the age-grades of the northern Plains Indians (*Altersklassen und Männerbünde*, p. 154). The argument just advanced in behalf of age-societies is obviously applicable to the type of religious societies specifically mentioned by Dr. Goldenweiser, as well as to still other forms of social units. Is their exclusion justifiable from a point of view that emphasizes merely the *relation* of elements entering into a "totemic" complex?

In advancing these comments, the writer is fully aware of the fact that he may not have fully grasped Dr. Goldenweiser's meaning. The subject of totemism is not yet quite in the position of those metaphysical problems of which Clifford has said that, in discussing them, people find it peculiarly difficult not only to make out what another man means, but even what they mean themselves. But that it is peculiarly difficult to discover another man's conception of totemism is amply attested by the recent history of ethnology. However this may be, Dr. Goldenweiser

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himself knows quite well that his analytical study is not definitive, but programmatic; that the next step must be a more extensive ethnographic investigation of the field. What he has already given is a statement of first principles. Whatever deficiencies may be found in his definition, he has been the first to show at length, and with irrefragable logic, that totemism can not be treated as an integral datum,—the first, as already stated, to apply the doctrine of secondary association to the subject of his inquiry. From this point of view, his paper constitutes a landmark in the history of totemic study,—the prolegomena to all positive attempts at a sane interpretation of “totemic” institutions.

On the Principle of Convergence in Ethnology

GRAEBNER'S POSITION

IN A RECENT WORK ON THE METHODS OF ETHNOLOGY,¹ DR. Graebner once more expounds the theoretical position familiar to readers of his former writings.² The central problem of ethnology is for him the determination of cultural connections. Resemblances in culture must be primarily accounted for by historical connection,—in the first place, because the existence of such connection stands unchallenged for a large part of the phenomena; secondly, because there are no objective criteria of independent development. Lack of historical relationship cannot be established by the most intense feeling that such a relationship is improbable, for this feeling is of a purely subjective character. Neither can the absence of proof for historical connection be interpreted as a stringent demonstration that an

Presented at the annual meeting of the American Folk-Lore Society in Washington, December 28, 1911, and printed in *Journal of American Folk-Lore*, XXV (1912), 24-42.

¹ Fritz Graebner, *Methode der Ethnologie* (Heidelberg, 1911).

² More particularly, *idem*, "Die melanesische Bogenkultur und ihre Verwandten," *Anthropos*, IV (1909), 726-780, 998-1032.

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historical relationship does not exist. It is indeed conceivable that, after determining all cultural relationships, we may still be confronted with independent partial similarities; but obviously this conclusion would result, not from the application of definite criteria of independent evolution, but solely from the non-applicability of the criteria of cultural connection. "So bleibt denn als erstes und Grundproblem der Ethnologie wie der ganzen Kulturgeschichte die Herausarbeitung der Kulturbeziehungen."

What, then, are the criteria of cultural connection? Two such are recognized by Graebner,—the criterion of form, that is, of the coincidence of characteristics not necessarily resulting from the nature of the objects compared; and the criterion of quantitative coincidence. In innumerable cases the form-criterion is self-sufficient. Nevertheless, Graebner notes instances of its misapplication, through fanciful affiliation of heterogeneous forms. Here, it seems, the quantitative criterion should have been used; that is to say, as it is one of the cardinal doctrines of Graebner's philosophy of ethnology that the diffusion of isolated cultural elements—even of myths—is impossible (*kulturgeschichtliches Nonsense*), the doubtful parallelism of two forms can be immediately established if they are recognized as elements of the same or related cultural complexes. So far as continuous areas are concerned, these criteria have not been challenged: they are generally employed in establishing linguistic relationship, and have proved valid in the study of European culture. Graebner sees no reason for limiting the criteria to continuous areas: he does not hesitate, for example, to use them as proofs for a far-reaching connection between Old World and New World culture. The only objection advanced against such applications of the criteria has been the improbability, under primitive conditions, of diffusion over the tremendous distances dealt with. On the one hand, this argument is refuted by the migrations of the Malayo-Polynesians and the occurrence of Asiatic tales in South America. But, in addition, the contrary argument may be strengthened by two auxiliary principles. The supposed lack of continuity between two areas may prove deceptive. There may be found cultural features bridging the geographical gap between

the areas compared (continuity-criterion); and there may be such a diffusion of cultural elements, that geographical proximity varies directly with the degree of cultural relationship (criterion of form-variation),—a result manifestly not to be expected on the theory of independent evolution of parallel forms.³

The foregoing account already describes by implication Graebner's position on the subject of convergent evolution. From his point of view, it matters little whether similarities are believed to result from a psychology common to mankind or from the convergence of originally distinct phenomena. In either case, there is an assumption of independent development; and as positive criteria of independent development are, according to Graebner, non-existent, both theories are on a methodologically inferior plane as compared with the doctrine of historical connection. In particular, Graebner criticises Ehrenreich's definition of "convergent evolution" as the result of similar environment, similar psychology, and similar cultural conditions. Similarities in natural conditions, he contends, have been considerably overestimated. The psychology of different branches of mankind shows as much differentiation as their physical traits. As a matter of fact, the psychological unity of mankind, which is invoked to explain cultural resemblances, has really been inferred only from the observed resemblances. If peoples of distinct geographical areas reveal far-reaching psychical resemblances, the question arises whether these are not ultimately due to genetic relationship or cultural contact. So far as the similarity of cultural conditions is concerned, Graebner insists that, if independent development be assumed, similarity of cultural conditions could result solely from the natural environment, and that similarity of cultural conditions would presuppose a high degree of psychical resemblance. Against Ehrenreich's statement that in spite of various parallels with Old World culture the culture of America bears a distinctively American stamp, Graebner declares that it is not clear how heterogeneous cultural conditions could lead to parallels, which, according to Ehrenreich, must be due to similar cultural environment. An *a fortiori* argument is used to clinch the discussion. European civilization

³ Graebner, *Methode der Ethnologie*, pp. 94-125.

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has developed a remarkable similarity of cultural milieu. Nevertheless the number of well-authenticated instances of independent parallel development is exceedingly small. In the majority of instances we find merely combinations of thoughts and motives already extant in the culture common to authors, inventors, or thinkers. But even the residual cases lose their force as to convergent development among primitive races: for, on the one hand, these modern instances rest on a peculiarity of modern culture,—the conscious striving for progressive development; on the other, the same thought may indeed be *conceived* twice, but the literature of science indicates that the same thought does not necessarily become socially and culturally significant in more than one case. If a cultural similarity resting on close genetic relationship has produced so small a number of independent parallels of social significance, it may reasonably be doubted whether the relative psychological unity of mankind, and the resemblance of natural conditions, could produce such absolute identity of culture as to result not merely in the conception, but in the social acceptance and further development, of the same thoughts.

Two questions confront the reader in connection with the views presented above. In the first place, does Dr. Graebner correctly define the logical standing of the antagonistic theories of independent development and genetic or cultural relationship? Secondly, does Dr. Graebner grasp the essentials of the doctrine of convergence as it has been employed in ethnological practice? The following pages will be devoted to an examination of these questions.

LOGICAL STANDING OF THE RIVAL THEORIES

The supposed methodological superiority of the theory of contact and relationship rests, as indicated above, on the assumption that it is distinguished by positive, objective criteria, while the rival theory lacks such criteria.⁴ Indeed, the argument that independently evolved cultural similarities could be detected only by the non-applicability of Graebner's criteria (p. 107)

⁴ This point of view also appears in Graebner's brief reply to a critique by Arthur Haberlandt in *Petermanns Mitteilungen*, 1911, pp. 228–230.

involves the strongest conviction that criteria of independent development not only have not been found, but that it is impossible to discover them.

In the first place, the objectivity of Graebner's criteria is in large measure illusory. He himself points out that the form-criterion is liable to fanciful subjective interpretations (p. 118). In all doubtful cases, however, he counsels testing by the second, unconditionally objective (*unbedingt objektiven*) criterion of quantity. It may at once be admitted that this criterion does provide a quantitative measure for the degree of relationship between two cultural complexes. This relationship, however, cannot be established except by demonstrating the relationship of corresponding elements in the two complexes. Each equation can be made only by the application of the form-criterion. In each particular comparison there will thus admittedly be a subjective factor, hence it is quite illogical to argue that a summation of parallels will eliminate the subjective element. Apart from this, what we know of the psychology of investigation does not justify us in the belief that a student who discovers intensive morphological resemblances—though other investigators fail to note them—would ever feel the necessity of resorting to a test by another criterion; and, if he did, he doubtless would have little difficulty in propping up his fanciful parallel by others not less whimsical. Indeed, the quantitative test leads to curious results in Graebner's own case. Against Haberlandt,—who reproaches him with classifying together such diverse objects as the "male" and the "female" spear-thrower, nay, even the Maori sling-stick,—Graebner urges that, if a complex has once been established on the basis of well-defined elements, even a morphologically indeterminate element, such as the spear-thrower, must be regarded as part of the complex, provided its distribution coincide with that of the other elements.⁵ This is undoubtedly a vicious principle. From the identity of even an indefinitely large number of corresponding elements in two series it does not follow that certain other associated elements are genuine parallels and must be brought into a genetic relation-

⁵ *Ibid.*, p. 229. Graebner, of course, does not neglect the differences in spear-thrower types except in his theoretical speculations (see *Anthropos*, IV, 736).

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ship. The "male" and the "female" spear-thrower might reasonably be grouped together as conceivable differentiations from a common prototype; but to argue that so heterogeneous an object as the sling-stick is related to them if it occurs in a similar combination of elements is not testing the criterion of form, but sacrificing it.

While Graebner's criteria of genetic relationship are thus found to lack the strictly objective character claimed for them, independent development need not be defended on purely subjective grounds, even where a stringent demonstration is impossible. Graebner criticises Ehrenreich for holding that the same mythological ideas may develop independently a great number of times from universally observable natural phenomena.⁶ This, he contends, is an a priori position lacking in sanity, because from the ready conceivability of independent development we cannot infer the *fact* of independent development (p. 97); that is to say, Graebner considers the theory of independent development inferior, because it leaves the door open to the arbitrary individual judgment of psychological probability. Now, it may at once be admitted that no amount of psychological investigation can actually demonstrate that two given cultural phenomena, possessing as they do the unique character distinctive of historical happenings, originated independently. A demonstration could be given only if we knew the actual history, which we generally do not. As a matter of fact, however, the theory of independent development is not one whit worse off in this respect than its rival theory; for it is an utterly mistaken notion that the psychological factor is excluded by the assumption of cultural relations. The comparison of form can never do more than establish the identity of forms; that such identity is to be explained by a genetic relationship is an hypothesis of varying degrees of probability. That the details of the crutch-shaped Melanesian paddle should occur in South America is to Dr. Graebner a sufficient proof of common origin (p. 145). Why? Because he cannot conceive how such similarity could result independently. But what is inconceivable for him is perfectly conceivable for Ehrenreich and others. From the inconceivability of independ-

⁶ Paul Ehrenreich, *Die allgemeine Mythologie und ihre ethnologischen Grundlagen* (Leipzig, 1910), p. 266

ent development by a single student we certainly cannot infer the fact of a common origin. We are dealing with probabilities, not with certainties in either case; the only point is to increase the probability of either theory, and here I cannot find that the doctrine of independent development is in a less favorable position. It seems to me, on the contrary, that a number of observations in individual psychology, as well as a number of social facts, well-nigh establish the independent development of certain simple cultural traits; and that in other cases the probability of such development, while not as yet determined, can be readily investigated at the present time.

As an example of the former kind I should regard certain observations on the reactions of children in the dark. If the widespread fear of the dark which enters into primitive beliefs were exclusively the result of tradition, it might be reasonably argued that it had developed from the same source of origin. This theory, however, becomes improbable as soon as we find that the distinctive feeling of uncanniness appears in equal force where all traditional beliefs tending to foster dread of the dark have been rigorously excluded from the child's curriculum.⁷ An element not altogether negligible in primitive belief is thus shown to be an element of our psycho-physical constitution. The psychology of dreams furnishes additional material bearing on the question. If certain physiological conditions, say retinal irritations, are regularly correlated with certain dream images which coincide with widespread mythological conceptions, then such conditions must be considered as constituting a *vera causa* for the explanation of the mythological ideas. Thus, the widespread conception of a grotesquely distorted countenance may be plausibly traced to Wundt's "*Fratzenträume*." Of course, we do not know, and never shall be able to know with certainty, that these dreams formed the foundation of the corresponding beliefs. But to disregard them entirely, to deny that they affect the merits of the case, would be to indulge in that form of sterile hypercriticism with which Graebner not infrequently reproaches his own opponents. In other directions, systematic

⁷ Ernst Mach, *Die Analyse der Empfindungen* (Jena, 1906), p. 62. These observations were confirmed by Dr. Petrunkevitch in an oral communication to me.

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observations could at least be planned and instituted. For example, psychological child-study might establish the fact that children of different countries react in an essentially similar way on the everyday phenomena observable in the heavens. With the same reservations as before, due to the unique character of historical happenings, we should then be justified in attaching a high degree of probability to Ehrenreich's conjecture as to the independent origin of simple nature myths. In other fields, the study of individual psychology from this point of view might present greater practical difficulties: it might, for example, prove impossible to disentangle the influence of traditional art-forms in an inquiry into the development of drawing and design. On the other hand, the inquiry into types of association, such as Galton was the first to conduct on a large scale, seems full of promise, especially so far as color and number symbolism are concerned. The contention that an apparently very odd association common to two distinct regions must have travelled from one to the other must immediately lose its force if we find the same association arising with a certain frequency among ourselves. The objection might indeed be raised that, in order to become a cultural phenomenon, the individual association would have to be socialized; this would, however, apply in equal measure on the supposition of borrowing.

So far, then, as the objectivity of the criteria is concerned, the inferiority of the theory of independent development stands unproved. In determining genetic relationship on the ground of formal resemblance, the influence of the personal equation is unavoidable; on the other hand, the arbitrariness of speculations on independent development can be limited by the results of scientific (as opposed to popular) psychology.

If there is any difference in the value of the two theories, it must rest on the alleged absence of historical proofs for independent development, in the face of the universally admitted existence of such proofs for historical connection. It remains to be shown that this allegation is erroneous, that there exist unexceptionable instances of convergent evolution. For this purpose it is necessary to examine somewhat more closely the concept of convergence.

DEFINITION OF "CONVERGENCE"

The fundamental error in Graebner's critique of convergent evolution lies in the fact that it entirely ignores the group of phenomena to which the principle criticised has been most successfully applied. Taking into account only Ehrenreich's *definitions* of "convergence," and disregarding completely Ehrenreich's further remarks on the subject, Graebner is led to reject the theory because, for the explanation of identities, it seems to involve the assumption of a mystic psychological unity (p. 145).

To be sure, it must be admitted that, if we found *exact* parallels of very complicated phenomena, their occurrence in two areas, no matter how widely separated, could not reasonably be explained by convergence. Let us assume for a moment that we found on the northwest coast of America a social system duplicating such Australian elements as four-class exogamy, belief in lineal descent from the totem, elaborate rites for the multiplication of totems, and the like. If this were the fact, an explanation by the psychic unity of mankind would be lamentably deficient, as may readily be shown by examination of a concrete case. Ehrenreich writes, "Wo gleiche Geistesanlage sich vereint mit Gleichheit der Wirtschaftsform und der gesellschaftlichen Stufe, wird die Cultur im Allgemeinen überall einen gleichen Charakter, einen gleichen Typus tragen, und wir dürfen uns nicht wundern, wenn solche gleiche Typen auch in Einzelheiten grosse Übereinstimmung zeigen und Convergenzen hervorbringen."⁸ Let us test the explanatory value of the principle, as thus defined, by a single example. Ehrenreich finds a surprising resemblance between the Dukduk masks of New Britain and the Fish-Dance masks of the Karaya, as well as between the correlated usages. Granting the resemblance, nay, even the exact identity, of the features in question, what meaning can we associate with the statement that the parallel is due to psychic resemblance linked with like economic and sociological conditions? The identity to be explained is not found except among the two above-mentioned representatives of two distinct racial types. What are the psychic traits and cul-

⁸ Paul Ehrenreich, "Zur Frage der Beurtheilung und Bewerthung ethnographischer Analogien," *Correspondenz-Blatt der deutschen Gesellschaft für Anthropologie, Ethnologie und Urgeschichte*, 1903, pp. 176-180.

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tural conditions common to these two tribes, which are *not shared by those of their geographical neighbors and racial congeners lacking the cultural homologues under discussion?* The principle of continuity is in fact not less essential to a sane theory of independent development than to a sane theory of transmission. There is at least no logical difficulty in assuming that certain laws of evolution are immanent in human society, and must lead *everywhere* to the same results. But to say that psychic affinity and cultural similarity have produced in two or in a few instances the same result is logically admissible only if it be shown at the same time for what specific reasons the same result is not noticeable in all other cases, even where psychic affinity is reinforced by racial relationship, and cultural affinity by geographical and historical contact. So far, then, as Graebner's attack is directed against Ehrenreich's explanation of supposed identities, it is entirely justified: such an explanation is indeed nothing but a mystification. Granted the existence of identities, they are inexplicable.

But the entire aspect of the question changes if we do not interpret the given parallels as identical or homologous, but merely as analogous. In the brief but profound paper quoted above, Ehrenreich has treated this problem with the greatest possible clearness. Over and above what he regards as genuine convergences, he distinguishes "false analogies," due to the inadequacy of our knowledge, to the premature classification of diverse traits under the same concept, labelled with the same catchword. It is merely necessary to conceive all parallels of any degree of complexity as "false analogies,"—to explain them as Ehrenreich himself explains, in exemplary manner, the various forms of totemism, of the belief in metempsychosis, of the swastika and eye-ornament,—and the mystical element in the theory of convergence disappears. The observation of similarities, especially in the absence of obvious paths of diffusion, then leads directly to the query whether the similarities are not purely classificatory, and hence, from the standpoint of genetic relationship, illusory.

In a review of Graebner's recent book, which has been published since the writing of the preceding paragraphs, Professor Boas says,

Nobody claims that convergence means an absolute identity of phenomena derived from heterogeneous sources; but we think we have ample proof to show that the most diverse ethnic phenomena, when subject to similar psychical conditions, or when referring to similar activities, will give similar results (not equal results), which we group naturally under the same category when viewed, not from an historical standpoint, but from that of psychology, technology, or other similar standpoints. The problem of convergence lies in the correct interpretation of the significance of ethnic phenomena that are apparently identical, but in many respects distinct; and also in the tendency of distinct phenomena to become psychologically similar, due to the shifting of some of their concomitant elements—as when the reason for a taboo shifts from the ground of religious avoidance to that of mere custom.⁹

As is shown by a preceding quotation from Ehrenreich, Professor Boas goes too far in his initial statement, for Ehrenreich's conception of genuine convergence does practically involve a belief in an absolute identity derived from heterogeneous sources; but his utterance indicates that in America, at all events, convergence has been treated in a manner which entirely escapes Graebner's attention.

It is now necessary to discuss convergence as resulting from modes of classification, to show what form of classification gives rise to the appearance of identical results from diverse sources, and to illustrate the point by a number of special instances.

PREMATURE CLASSIFICATION

Premature classification appears in ethnological literature in two principal forms: the ethnologist may either infer from the undoubted identity of certain elements in two different complexes that the complexes themselves are identical; or he may fancy identity of elements or complexes where none exists. The first type of premature classification has wrought considerable mischief in the consideration of ceremonial complexes, such as the Midewiwin and the Sun Dance. The psychology of this fallacy is not unlike that of illusions. A complex such as the Midewiwin is described for some particular tribe; and some conspicuous feature, say, the shooting-ritual, acquires a symbolic function; so that

⁹ Franz Boas' review of Graebner, *Methode der Ethnologie*, in *Science*, XXXIV (1911), 804-810; the quotation is from p. 807.

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whenever this feature appears in another tribe, it is at once supposed to indicate the presence of the residual elements of the complex first described. This would indeed be a justifiable inference, if a complex invariably represented a quasi-organic unit; but this is precisely what is not ordinarily the case. For example, Dr. Radin has recently shown¹⁰ that the Midewiwin of the Winnebago and that of the Central Algonkian are not identical, because in each there has been a secondary association between the common elements and a preponderant group of specific elements, which in large measure can be shown to result from the specific character of Central Algonkian and Winnebago culture respectively. I have suggested elsewhere¹¹ that what Dr. Radin has successfully demonstrated for the Midewiwin applies in like measure to the Sun Dance of the Plains tribes. We cannot reduce to a common prototype the various forms in which the ceremonies grouped under this catch-word appear. All we can do is to ascertain the relatively few common elements which have acquired the symbolic function mentioned, and to investigate their varying combinations in different cases.

It is clear that the form of erroneous classification treated above, however large it may loom in ethnological discussion, has nothing to do with convergent evolution; for in the cases mentioned the genetic relationship of the identical features has never been challenged, while apart from these features there is obvious *divergence*. It is Ehrenreich's group of "false analogies" that supplies us with illustrations of the second type of classificatory error, and this has a direct bearing on the principle of convergence.

Comparing the two types of inadequate classification, we may say that the first type involves the assumption that an organic relationship exists where it does not exist, while the second type of error results from the failure to note that the supposedly parallel elements are organically related to two distinct complexes. In this latter case, then, the parallelism is between logical abstractions rather than between psychological and ethnological realities. Some concrete illustrations will make the matter clearer.

¹⁰ Paul Radin, "The Ritual and Significance of the Winnebago Medicine Dance," *Journal of American Folk-Lore*, XXIV (1911), 149-208.

¹¹ R. H. Lowie, *The Assiniboine*, AMNH, Anthropological Papers, IV (1909), 77 ff.

Owing to their theoretical interest, the so-called age-societies of the Plains may properly serve to introduce the subject. J. O. Dorsey reports that among the Omaha there were three feasting societies, composed of old men, middle-aged men, and youths respectively. In tribes of the same cultural area (Arapaho, Blackfoot, Mandan, Hidatsa) other writers have found series of dancing societies evincing a more refined classification by age, admission into any one society being contingent on a payment. Schurtz assumes that the existence of age-grades among the Omaha and other Plains tribes is due to an innate tendency of human society towards an age-grouping, which leads everywhere to similar results. From Graebner's point of view, the existence of so marked a feature as age-grades in a practically continuous area must be explained as due to historical connection. If, on the other hand, we here applied the principle of convergence in the sense defined by Ehrenreich, we should say that the resemblance between the Omaha age-classes and the age-societies of the other Plains tribes is due to the union of general psychic and specific cultural similarities of all the tribes concerned.

As a matter of fact, each of these three interpretations is erroneous. The Omaha feasting organizations are age-classes properly so called; that is to say, a man belongs to one of the three classes by virtue of his age. But the fact that, say, the Hidatsa societies present the appearance of age-classes is due to the mode of purchase obtaining in this tribe. The age factor is indeed active, inasmuch as it is customary for age-mates to purchase a society in a body; but there is no established division of Hidatsa society into age-grades, no correlation between age and membership in a certain definite organization. The correlation is, instead, between membership and *purchase*: a Hidatsa belongs to every society of the series which he has purchased, but which has never been purchased of him. A man of ninety may thus hold membership in a young men's society, and under abnormal circumstances a group of men may acquire a membership which ranks superior to that of an older age-group. To call both the Omaha and the Hidatsa organizations "age-societies" is therefore admissible only if we regard this term as a convenient catch-word which may denote neither psychologically nor genetically related phe-

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nomena. The age-factor that we isolate in studying the Hidatsa system is, of course, as a logical abstraction comparable to corresponding abstractions, whether derived from the Omaha system or that of the Masai. In reality, however, it forms part of a context which determines it, and from which it cannot be wrested without completely altering its character. What we find in comparing the Omaha and the Hidatsa systems is therefore a convergence of a type different from that defined by Ehrenreich, but coinciding absolutely with that of his "false analogies," which result from our relative ignorance of the phenomena compared. So long as we knew only that the Hidatsa had societies composed of men of different ages, it was possible to classify them as age-grades proper. With the additional knowledge of the subjective attitude of the natives towards these societies, the justification for such a classification disappears.

What has just been shown for age-grades may be similarly shown for the much-discussed phenomenon labelled "exogamy." It has commonly been assumed that the regulation against marriage within a certain group, no matter in what part of the globe such a regulation may be found, is uniformly the same in principle. Dr. Goldenweiser has recently shown that this is by no means the case. Clan exogamy may indeed be the expression of the feeling that marriage within the clan as such is incestuous; but it may also, as among the Toda and Blackfoot, be a secondary development, the fundamental fact being an objection to marriages between blood relatives. From Dr. Graebner's standpoint there is no reason to differentiate between the primary and the secondary type of clan exogamy. The form-criterion merely tells us that two groups are both exogamous; that in point of exogamy they are identical, and in so far may reasonably be supposed to be genetically related. So far as the criterion of quantity is concerned, nothing would be easier than to bolster up the parallel exogamy by other resemblances. Thus, the Crow social units, which exemplify the clan of "classical" ethnological literature in being exogamous in their own right, bear nicknames of similar type to that of the Blackfoot. Here again the identity of the facts compared is logical, while the facts we are really interested in studying are psychological. The exogamous conduct of the

Blackfoot is inseparably linked with his feeling towards blood relatives; the exogamous conduct of the Crow is part of a quite distinct psychological complex. Only by disregarding the characteristic features of exogamy in these two instances do we get an identical *Gedankending*.

In this connection it is interesting to discuss the two-phratry system (*Zweiklassensystem*), as Graebner himself makes an extensive use of this concept, suggesting, for instance, an historical connection between the two-phratry organization of Oceania and that of the Northwest Coast Indians and the Iroquois.¹² Before considering such a suggestion, we should have to be convinced that the term "two-phratry system" invariably labels the same phenomenon. Serious doubt is thrown on such a supposition by a consideration of the data collected by Rivers among the Toda. In this tribe the numerical preponderance of one clan is such that its members can follow the exogamous rule only by marrying most of the members of the other clans, "leaving very few to intermarry with one another." Out of 177 marriages, only 16 were between members of the other clans. As Rivers recognizes, there has thus developed the closest conceivable approximation to a two-phratry system.¹³ Yet this result has been achieved by unique historical causes quite distinct from those which brought about such a system where there are merely two intermarrying phratries without any lesser exogamous units.

An instance of similar suggestiveness is furnished by the recent history of the Crow. A visitor to this tribe some forty years ago would have found the male members of the tribe grouped in two social units,—the Foxes and the Lumpwoods. Without any real feeling of mutual hostility, these two units were constantly pitted against each other; for example, taking opposite sides at games, and constantly attempting to outdo each other in warlike deeds. To a superficial observer this division would have appeared similar to that of the Iroquois phratries, though, as a matter of fact, the Lumpwoods and Foxes were not social units with inheritable membership, but military societies. At all events, even a

¹² Graebner, "Die melanesische Bogenkultur . . .," *Anthropos*, IV, 1021.

¹³ W. H. R. Rivers, "Totemism, an Analytical Study," *Journal of American Folk-Lore*, XXIII (1910), 246.

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more careful investigator might have been struck by the phenomenon as one comparable with the tendency to the formation of dual divisions, as evidenced in civilized life by the frequency of two dominant political parties. Nevertheless, forty years prior to the hypothetical investigator's advent, he would have found no less than eight societies of the same type.¹⁴ A detailed study of the development of military societies among the Crow shows beyond a doubt that the presence of but two military organizations forty years ago was not due to a primary dual organization, but came about solely through the elimination of the other organizations. A comparison of the Crow conditions with those still more recently found among the Gros Ventre is of the utmost interest. In this tribe the old ceremonial grouping of the men in a rather large number of small companies representing probably six age-grades has been completely superseded by a division into two organizations,—the War Dancers and the Star Dancers. The tribal and social functions of these societies bear close resemblance to those exercised by the Lumpwoods and Foxes of the Crow, and the spirit of rivalry is equally prominent in the Gros Ventre organizations. But while the dual grouping of the Crow men resulted from a process of elimination, precisely the reverse process took place among the Gros Ventre. The War Dance "is universally stated to be a recent importation from the Sioux, apparently within the present generation"; while the Star Dance is probably an old ceremony independent of the age-series.¹⁵ In the two cases under discussion, then, a dual grouping is beyond a doubt the result of convergent development.

To revert to Graebner's own concepts, we may next consider his category of drums with skin drum-heads.¹⁶ He is careful to enumerate the several Oceanian forms; but as soon as his extra-Oceanian speculations begin, differences of form seem to become negligible. The skin drum of the West African culture-area is de-

¹⁴ Maximilian Prinz zu Wied, *Reise in das innere Nord-America in den Jahren 1832 bis 1834* (Coblenz, 1839), I, 401.

¹⁵ A. L. Kroeber, *Ethnology of the Gros Ventre*, AMNH, Anthropological Papers, I (1908), 234-239.

¹⁶ Graebner, "Ein Element von sehr typischer Verbreitung bieten zum Schlusse noch die *Musikinstrumente* in der einseitig bespannten, meist sanduhrförmigen, bisweilen zylindrischen Felltrommel," *Anthropos*, IV, 770.

scribed as one of the elements connecting it with Melanesian culture. It is said to appear with all the characteristic modes of securing the drum-head,—viz., by thongs, pegs, and wedges, —though the hourglass shape of the instrument is less frequent.¹⁷

Probably it would be difficult to find a more offensive example of the misapplication of the form-criterion. The very reference to the hourglass-shaped forms of Africa involves an error of the worst kind. Graebner's authority defines the hourglass drum of Africa as composed of two skin-covered bowls connected by a cylindrical tube. Three sub-types are distinguished, of which two recall the shape of a dumbbell, while the third differs radically from the two others by the presence of four lugs and profuse decoration, and by the width of the connecting cylinder, which approximates that of the bowls.¹⁸ For convenience of description, Ankermann is certainly justified in creating an hourglass type. But it would be unjustifiable to draw any inference as to genetic relations between the third and the two other sub-types; for quite apart from the elaborate decoration and the four lugs, the third sub-type is not at all similar to the dumbbell form. It is a psychological commonplace that even congruous geometrical forms may produce very different psychological effects. It is a fact known to field-workers in America that identical patterns are sometimes not recognized by the natives as identical if executed in different colors. A fortiori, we cannot assume without proof, that, where the divergence of form is very great, the native still assembles the varying forms under the same concept. Artifacts differ from organic forms in lacking an innate tendency to variability. If, therefore, we suppose that the lugged (Barotse-Amboella) sub-type developed out of the dumbbell form, or vice versa, we introduce either the hypothesis that some external condition determined the change, or the psychological hypothesis that both forms were originally conceived as of one type. For neither of these suppositions is there the slightest foundation.

If the foregoing argument applies within even a relatively continuous area, its force surely does not diminish when "hour-glass drums" of different continents are compared. Indeed, the

¹⁷ *Ibid.*, pp. 1011 ff.

¹⁸ Bernhard Ankermann, "Die afrikanischen Musikinstrumente," *Ethnologisches Notizblatt*, III (1901), 98 ff., 53-55.

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hourglass drum of New Guinea, as described and pictured by Finsch, Biró, Schlaginhaufen, and others, bears no resemblance to the African sub-types. We must regard the term "hourglass drum" as merely a convenient classificatory device by which may be described objects of diverse origin. The geometrical abstraction defined by the term corresponds to no cultural reality; it develops in different areas by convergent evolution.

As a matter of fact, the hourglass type which at least presents a semblance of morphological classification plays a very subordinate part in Graebner's treatment of the skin drum; for under the category of skin drums—and accordingly as evidence of a cultural connection between Oceania and North America—are cited the ordinary dancing-drum and the Midewiwin drum of the Ojibwa.¹⁹ Thus the form-criterion is completely abandoned by its champion.

It is true that Dr. Graebner, in his treatment of this subject, attaches considerable weight to the method of securing the drum-head,—whether by thongs, pegs, or wedges (*Schnur-, Pflöck-, und Keilspannung*). This leads to an important question. How many ways of fastening a skin membrane to a drum are conceivable? Very little reflection is required to show that the number is exceedingly limited. Indeed, the wedge system, being only a sub-type of the *Schnurspannung*, is not entitled to a special position on logical grounds, though from a comparative point of view it is incomparably the safest criterion of relationship. We must here apply what Dr. Goldenweiser has called, in conversation with the author, "the principle of limited possibilities," which has recently been thus defined: "The theory of convergence claims that similar ways *may* (not *must*) be found. This would be a truism if there existed only one way of solving this problem; and convergence is obviously the more probable, the fewer the possible solutions of the problem."²⁰ In the case at hand, it cannot be taken as a sign of genetic connection that some African and some Oceanian tribes use pegs for fastening a drum-head, because the number of available ways is very small *if classified in a manner that abstracts from all definite characteristics*.

This point is illustrated most clearly where the logical classifica-

¹⁹ Graebner, "Die melanesische Bogenkultur . . .," p. 1021.

²⁰ Boas, *op. cit.*, p. 807.

tion involves a dichotomy of the universe. A well-known writer has discussed the origin myths of primitive folk, and found that some involve a theory of evolution, others one of special creation. No sane ethnologist would infer from this that all the myths of either type were historically connected. To choose a somewhat more drastic illustration. Acquired biological traits must either be inherited or not inherited: consequently an expression of opinion, whether consciously or unconsciously bearing on the subject, must fall into either category. Many primitive tribes have myths recounting how in the remote past a certain animal met with some adventure which caused it to assume some biological peculiarity now noticeable in its descendants; nevertheless it would be absurd to accept this tacit assumption of transmission as a parallel of anti-Weismannism. Countless examples of a mode of classification rivaling in absurdity the hypothetical instance last cited are furnished by histories of philosophy. Too frequently the historian utterly neglects the processes by which conclusions are reached, and groups thinkers exclusively by the nature of their conclusions, which are labelled by descriptive catch-words. The identification of a philosopher as a monist or dualist, idealist or realist, is undoubtedly a labor-saving mode of characterization; but unfortunately it precludes a deeper comprehension of the thinker's philosophic individuality. A differentiation of social systems on the basis of maternal and paternal descent, such as Graebner has undertaken, is justifiable within a limited area, where historical connections can be definitely demonstrated. Outside such an area it can have no comparative significance, because descent cannot be reckoned otherwise than in either the maternal or the paternal line, or in both.

THE POSSIBILITY OF GENUINE CONVERGENCE

The foregoing discussion has indicated the nature of the errors due to premature classification. The frequency of such errors, and the readiness with which they are committed, surely justify the greatest caution in identifying apparent homologies in the cultures of tribes not known to be historically related. The first question we must ask is, not how the trait could have travelled from one region to another, nor even whether it could have

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originated independently through the psychic unity of mankind. Our first duty is rather to ascertain whether the resemblances are superficial or fundamental. For example, if we discover that the *manang bali* of the Sea Dyaks corresponds in the most striking manner to the *berdache* of the Plains Indians,²¹ we should not straightway identify the two institutions and invoke the principle of psychic unity or that of historical connection. Psychic unity would only explain the fact of a pathological variation, which seems to occur everywhere with a certain frequency. It does not explain why in but two particular areas this variation should lead to a marked social institution. Neither can historical connection be postulated in the absence of a tittle of evidence for either genetic relationship or transmission. The advocate of convergence in the sense here proposed will simply await a fuller determination of the facts. If closer investigation should establish an absolute identity, the fact of identity would stand, but would stand unexplained.

But in many instances the identity of the cultural elements compared seems to be far more than an abstract possibility. The eye-ornament of the northwest coast of America is identical with that of Melanesia. For all practical purposes the star-shaped stone clubheads of New Guinea are identical with those from Peru. To put the case in the most general form, wherever we are dealing with objects which can be fully determined by an enumeration of their visible or sensible traits, there is the possibility of proving objective identity, as indicated by the examples just cited. However, there is an important consideration which cannot be neglected in this connection. The sensible traits of an ethnographic object may completely determine its character from the standpoint of the curiosity-dealer, but never from that of the scientific ethnologist.²² For the latter a material object has a purely symbolical function: it represents a certain technique, an artistic style, a religious or social usage. In this sense it may be rightly said that "material" culture does not exist for the eth-

²¹ E. H. Gomes, *Seventeen Years among the Sea Dyaks of Borneo* (Philadelphia, 1911), pp. 179 ff.

²² Cf. Franz Boas, "Some Principles of Museum Administration," *Science*, XXV (1907), 928.

nologist, for the very word "culture" implies a psychological correlate, or rather determinant, of the material object. According to Pechuel-Loesche, the same representation of a human figure that in one West African specimen is nothing but a product of art industry becomes, when endowed with certain magical powers by virtue of incantations or the application of sacred substances, a fetich. Exactly the same purpose, however, may be served in the same tribes by the most inconspicuous objects of nature. A purely objective comparison would here lead to an utterly erroneous classification. It would wrest the factors studied out of their organic context in quite the same way as an identification of the cultural traits discussed in the preceding section; it would neglect the very factors that we are most interested in studying.

As has been pointed out by American archaeologists, the application of the form-criterion is insufficient in determining the antiquity of an archaeological object; for the latter may not be at all the completed object designed by the worker, but a mere "reject."²³ Yet objectively the rejects coincide absolutely with the finished products of a lower culture. The difference lies in the cultural contexts of which the objects are elements: the resemblance may be perfect from a purely external standpoint; nevertheless it represents, in Ehrenreich's terminology, not a genuine convergence, but a false analogy. A most suggestive fact pointing in the same direction has been ascertained in Central Australia. The natives of this area use implements, some of which fall morphologically under the category of paleoliths, while others are neoliths. Investigation has shown that this morphological difference is a direct result of the material available for manufacture. Where diorite is available, the natives manufacture "neolithic" ground axes, in other cases they make flaked implements practically as crude as those of the ancient Tasmanians.²⁴ The manufacture of "neolithic" implements in Central Australia and elsewhere thus forms another instance of convergence,—a classificatory resemblance due to heterogeneous conditions. It is true that Graebner

²³ J. Alden Mason, *The Origins of Invention*, p. 124.

²⁴ Baldwin Spencer and F. J. Gillen, *The Northern Tribes of Central Australia* (London and New York, 1904), p. 635.

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does not ignore the possible influence of material on form,²⁵ but he fails to show under what circumstances the ethnologist should seek to correlate morphological resemblance with the nature of the material. The form-criterion by itself does not tell us that diorite lends itself to "neolithic" workmanship, that bamboo bows are necessarily flat, that basalt furnishes the only material available for axe-manufacture in certain regions. Under what conditions should we be satisfied with formal coincidence as a proof of genetic relationship, and under what conditions should we inquire as to the possible influence of the available material?

The case of the eye-ornament adds force to the general argument. As Graebner might have learned from Ehrenreich's article (*op. cit.*, p. 179), Boas has shown that the eye-ornament of Northwestern America results from a peculiar style of art, which, so far as we know, does not occur in Oceania; that is to say, the objective identity is again deceptive, because it is an identity established by wresting a part of the phenomenon studied (the visible pattern) from the midst of its cultural context. Here it must again be stated that Graebner does not unqualifiedly uphold the omnipotence of the morphological principle. He rejects Von Luschan's speculations on the head-rests of New Guinea; he regards Schurtz's theories of the eye-ornament as "weniger phantastisch, aber doch auch übers Ziel geschossen"; he stigmatizes Stucken's attempt to trace all celestial myths to Babylon as an example of the neglect or unmethodical application of the form-criterion (p. 118). Unfortunately, he does not explain what is meant by an unmethodical or fantastic application of the form-criterion. As has been shown, the criterion of quantity is a measure of the historical connection between cultures, but can never decide as to the identity of doubtful traits. If all the other elements of Oceanian and northwest American culture were identical, the fact would prove nothing as to the identity of the eye-ornament in the two areas.

We are not always, indeed we are very rarely, in the fortunate position of knowing most of the determining conditions of an ethnological phenomenon. In the case of the rejects, of the central Australian "neoliths," and of the eye-ornament, we happen

²⁵ Graebner, *Methode der Ethnologie*, pp. 145, 117.

to be in possession of the facts; and from these instances we learn that morphological identity may give presumptive, but does not give conclusive, evidence of genetic relationship. It is conceivable that if we could determine the history of the South American paddles, which Graebner connects with Indonesian and Melanesian patterns,²⁶ we should find them to be genetically related; but we cannot bar the other logical possibility of independent origin, for it is likewise conceivable that each of the homologous features of the paddles originated from distinct motives and distinct conditions.

CONCLUSION

The doctrine of convergence, as here advocated, is not dogmatic, but methodological and critical. It does not deny that simple ethnological phenomena may arise independently in different regions of the globe, nor does it deny that diffusion of cultural elements has played an important part. It does not even repudiate the abstract possibility of the independent origin of complex phenomena (genuine convergence of Ehrenreich), though so far the demonstration of identities of such a character seems insufficient, and their existence would be unintelligible. The view here propounded demands simply that where the principle of psychic unity cannot be applied, and where paths of diffusion cannot be definitely indicated, we must first inquire whether the supposed identities are really such, or become such only by abstracting from the psychological context in which they occur, and which determines them,—whether, that is to say, we are comparing cultural realities, or merely figments of our logical modes of classification. A rapid survey of the field has sufficed to show that in many cases where some would invoke the principle of psychic unity, and others that of historic connection, the problem is an apparent one, which vanishes with a better knowledge and classification of the facts.

Dr. Graebner's ambitious attempt to trace historical connections between remote areas cannot be dismissed wholesale, on the basis of the foregoing criticisms. What has been shown is simply the

²⁶ *Ibid.*, p. 145; *idem*, "Die Melanesische Bogenkultur . . .," *Anthropos*, IV, 763, 1016, 1021.

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necessity for a critical use of ethnological concepts, and their occasionally quite uncritical use of Graebner. Even tangible specimens, it appears, cannot be studied apart from the culture of which they are a product. In the investigation of social and religious usages, where the subject-matter is itself psychological, the exclusive consideration of the form-criterion, to the detriment of the subjective factors involved, can lead only to disastrous results. Ethnology is a relatively young science, and it is natural that the mode of classification in vogue among ethnologists should have a pre-scientific tang. But the time has come to recognize that an ethnologist who identifies a two-class system in Australia with a two-class system in America, or totemism among the North-western Indians with totemism in Melanesia, sinks to the level of a zoölogist who should class whales with fishes, and bats with birds.

Ceremonialism in North America

IN DELIMITING THE RANGE OF CULTURAL PHENOMENA TO which this paper will be confined, it is impossible to adhere to any of the current definitions of "ceremony" or "ceremonial." A set mode of procedure is characteristic of every phase of primitive behavior, and thus it is justifiable to speak of birth, puberty, death, war ceremonies, etc. An article on "ceremonialism" in this sense would needs center in a discussion of the psychology of routine. When, however, Americanists speak of "ceremonialism," they generally associate with the term a more or less definite content of stereotyped form. Performances such as the Snake Dance of Pueblo peoples, the Sun Dance of the Plains, the Midewiwin of the Woodland area, are examples par excellence of what is commonly understood by a "ceremony." These performances are not individual, but collective undertakings; and, even where they hardly fall under the category of "religious observances" or "solemn rites," they are uniformly more than mere attempts at social amusement. As Indian dances are often performed for a serious purpose, or at least form elements of complexes of a serious character, the terms "dance" and "ceremony" are sometimes used interchangeably. This loose usage is as undesirable as the fre-

American Anthropologist, XVI (October-December, 1914), 602-631.

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quent identification of the problem of ceremonialism with that of organizations. There are North American dances performed exclusively as a matter of amusement, and there are organizations corresponding to our clubs rather than to ceremonial bodies. Elements of similarity may necessitate joint consideration of the ceremonial and non-ceremonial dances and societies; but it may be well to state that, in dealing with "ceremonialism," we start primarily from a consideration of solemn collective performances with an avowedly serious purpose, and shall include only such other phenomena as are historically or psychologically related to "ceremonialism" as thus defined.

Having regard to the limitation of space, a descriptive account of ceremonial activity in North America is out of the question here. I shall therefore merely enumerate the most important ceremonies in the several culture provinces, and shall then select for discussion a number of problems that arise from the consideration of our ceremonial data.

In the Eastern Woodland area, the Midewiwin looms as the most important ceremony of the Algonquian tribes, though its sphere of influence extended to several Siouan peoples, including some inhabiting the Plains. It was the property of a secret society, membership in which was preceded by a formal initiation. A shooting performance, either by way of initiating the novice or merely as a shamanistic practice, forms the most obvious objective bond between the forms of the ceremony as practised by the several tribes; while the interpretation of the aim of the ceremony varies.¹ The Iroquois also had a number of secret ceremonial organizations of as yet little understood character, of which may be mentioned the Little Water Fraternity and the False Face Society; the performances of the latter being characterized by the use of grotesquely carved face-masks. In addition, there was a series of tribal seasonal festivals, ostensibly in the nature of thanksgiving celebrations, held annually at such periods as the first flowing of the maple-sap, the planting and the ripening of the

¹ William Jones, "Central Algonkian," *Annual Archaeological Report for 1905* (Toronto, 1906), p. 146; Paul Radin, "The Ritual and Significance of the Winnebago Medicine Dance," *Journal of American Folk-Lore*, XXIV (1911), 149-208; W. J. Hoffman, "The Midewiwin or 'Grand Medicine Society' of the Ojibwa," *BAE, 7th Ann. Rept.* (1889), pp. 149-500.

corn, etc. These ceremonies, as well as the seven-days' New Year's Jubilee, correspond in a way to the spectacular composite performances of other areas in which religious practices are combined with entertainments of various forms.²

In the Southeast all other dances were completely overshadowed by the annual several-days' (from four to eight) festival known as the "Busk," and celebrated on the first ripening of the crops. The public making of new fire, the scarification of the men, and the taking of an emetic, are among the noteworthy objective features. The new-fire ceremony, as pointed out by Speck, has analogies not only in the Southwest, but even in Mexico; and the taking of an emetic is shared with some southern Plains tribes and the Pueblo Indians.³

In the Plains area, ceremonial activity attained a very high degree of development, though this was shared in very unequal measure by the several tribes. The Sun Dance, the great tribal performance of most of the inhabitants of the area, will be discussed below. Other ceremonial performances of wide distribution center in the rites connected with sacred bundles of restricted ownership. The widely diffused medicine-pipe ceremonials, the sacred-bundle rites of the Blackfeet, and the shrine performances of the Hidatsa, may serve as examples. There are mimetic animal dances, those in imitation of the buffalo occurring in varying guise and with varying *raison d'être*, such as the luring of the game. Some of the last-mentioned category of performances are the property of individuals who have experienced a vision of the same supernatural animal. Military and age societies, though in certain tribes wholly or predominantly secular, assume in others a markedly ceremonial aspect.⁴

² A. C. Parker and H. M. Converse, *Myths and Legends of the New York State Iroquois*, New York State Museum, Bull. 125 (1908), pp. 74 ff., 149 ff.; L. H. Morgan, *League of the Hodénosaunee, or Iroquois* (Rochester, 1854), pp. 187-222, 263-289.

³ F. G. Speck, *Ethnology of the Yuchi Indians*, University of Pennsylvania, Anthropological Publications, University Museum, I (1909), 112-131.

⁴ G. A. Dorsey and A. L. Kroeber, *The Arapaho Sun Dance*, Field Columbian Museum, Anthropological Series, V (1903), 1-228; G. A. Dorsey, *The Cheyenne*, same ser., IX (1905), 143-358; J. O. Dorsey, "A Study of Siouan Cults," BAE, *11th Ann. Rept.* (1894), pp. 351-544; Alice C. Fletcher, "The Hako: A Pawnee Ceremony," BAE, *22d Ann. Rept.* (1904), Part 2; A. C. Fletcher and Francis La Flesche,

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Among the Southwestern Indians, North American ceremonialism attains its high-water mark. There is a profusion of ritualistic externals,—wooden or sand-painted altars, prayer-offerings, masks, sacred effigies, and the like,—and esoteric fraternities perform elaborate ceremonies in order to heal the sick, or for the ostensible purpose of promoting the public welfare by effecting adequate rainfall or insuring success in the chase or war. These performances resemble the Iroquois festivals and the Plains Indian Sun Dance in being composite phenomena in which strictly religious features are blended with games, clownish procedure, and what not. The Hopi and Zuñi ceremonies further recall the Iroquois festivals in being calendric; that is, following one another in fixed sequence at stated seasons of the year.⁵

On the Northwest coast and its immediate hinterland we find the potlatch festival, involving a generous distribution of property by the host that entails a return distribution of gifts at a high rate of interest. Upon this secular basis there have been engrafted, among the northern tribes of the area, ceremonial concepts derived from the Winter Ritual of the northern Kwakiutl, from whose territory they have likewise extended southward. The Winter Ritual is founded on the novice's acquisition of a supernatural protector, whose character is in a measure predetermined by his family affiliations, or rather restricted by his family's supernatural property rights. During the winter, community of guardian spirits forms the bond of association, superseding family ties, and creating temporarily a number of ritualistic societies.

"The Omaha Tribe," BAE, 27th Ann. Rept. (1911); A. L. Kroeber, *The Arapaho*, AMNH, Bull., XVIII (1902-1907), 1-229, 279-454; *idem*, *Ethnology of the Gros Ventre*, AMNH, Anthropological Papers, I (1908) 141-282; R. H. Lowie, *The Assiniboine*, same ser., IV (1909), 1-270; *idem*, *Societies of the Crow, Hidatsa and Mandan Indians*, same ser., XI (1913), 143-358; Clark Wissler, *Ceremonial Bundles of the Blackfoot Indians*, same ser., VII (1912), 65-289; *idem*, *Societies and Dance Associations of the Blackfoot Indians*, same ser., XI (1913), 363-460.

⁵ J. W. Fewkes, "The Snake Ceremonials at Walpi," *Journal of American Ethnology and Archaeology*, IV (1894); *idem*, "The Group of Tusayan Ceremonials Called Katsinas," BAE, 15th Ann. Rept. (1897), pp. 251-313; Washington Matthews, *The Night Chant, a Navaho Ceremony*, AMNH, Memoirs, VI (1902); Matilda C. Stevenson, "The Sia," BAE, 11th Ann. Rept. (1894), pp. 16, 69-131; *idem*, "The Zuñi Indians," BAE, 23d Ann. Rept. (1904), pp. 62-283.

The ritual purports to portray the novice's abduction by the guardian spirits, their return to the village, and their restoration to a normal condition. In reality it is a compound of these elements with potlatch incidents, sleight-of-hand exhibitions, clownish activity, and so forth.⁶

Among the Eskimo unaffected by neighboring Indian peoples, ceremonialism apart from shamanistic practices is but slightly developed. The Central Eskimo have an annual festival that purports to effect the home-sending of the deity protecting the sea-mammals, and during which the shaman purges this deity's body by removing the effects of transgressed taboos. The appearance of masked performers impersonating the divinity and other spirits is a noteworthy trait of this ceremony.⁷

Paucity of ceremonial is a trait shared by the inhabitants of the Mackenzie area, the Plateau region, and California, all of whom present the least highly developed form of North American culture. Professor Kroeber has pointed out that the simpler the stage of culture the more important is the shaman.⁸ The statement might be extended from shamanistic practices to those practically universal observances connected with such events as birth, puberty, individual acquisition of supernatural power, and death. They, like the shamanistic functions in Kroeber's characterization, tend to become, "relatively to the total mass of thought and action of a people, less and less important." It thus seems possible to consider ceremonialism par excellence, as defined above and treated by preference in this article, a relatively recent trait superimposed on a series of simple routine procedures of the type just mentioned. The culture of the Mackenzie River people is relatively little known, but the prominence of shamanism and

⁶ Franz Boas, "The Social Organization and the Secret Societies of the Kwakiutl Indians," USNM, *Report for 1895*, pp. 311-737; *idem*, *The Mythology of the Bella Coola Indians*, Publications of the Jesup North Pacific Expedition, Vol. I (1898); J. R. Swanton, *Contribution to the Ethnology of the Haida*, same ser., Vol. V (1905); *idem*, "Social Conditions, Beliefs and Linguistic Relationship of the Tlingit Indians," BAE, 26th Ann. Rept. (1908), pp. 391-485.

⁷ Franz Boas, "The Central Eskimo," BAE, 6th Ann. Rept. (1888), pp. 583-609; *idem*, *The Eskimo of Baffin Land and Hudson Bay*, AMNH Bull., XV (1907), 119 ff., 489 ff.

⁸ A. L. Kroeber, *The Religion of the Indians of California*, Univ. Calif. Publ. Amer. Arch. and Ethn., IV (1907), 327.

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sleight-of-hand tricks appears clearly from Hearne's and Petitot's accounts;⁹ and among the Thompson River Indians the puberty ceremonials loom as a very important cultural feature.¹⁰ Shamanism with its correlated practices, and puberty rites, are known in other areas, but they are often eclipsed by the doings of esoteric brotherhoods and other spectacular performances. This is merely grazing a significant problem; and it must be clearly understood that, even in the ruder North American cultures, phenomena comparable to the more impressive ceremonials of other regions are not wholly lacking. Thus the Ute and related Shoshoneans celebrate an annual spring festival known as the "Bear Dance";¹¹ a series of winter dances with ceremonial raiment occurs among the Central Californian Maidu; and other Californian tribes have public annual mourning ceremonies and the semblance of a secret society formed by initiated male tribesmen.¹² The occurrence of these elements even in the simplest cultures seems to indicate rather clearly that the differences in ceremonial development are not correlated with psychological differences, but rather with differences in the manner of combining and multiplying elements of general distribution. A hint as to the luxurious growth of ceremonialism in certain areas will be found in the section on "Ceremonial Patterns," though why a certain feature extant in a number of regions should become a pattern in one tribe, and fail to become one in others, remains obscure.

Another question, which it is impossible more than to hint at here, relates to the distribution of ceremonial traits less widely diffused than those just dealt with. Thus ceremonial public confession is a trait shared by the Eskimo¹³ with the Iroquois¹⁴ and the northern Athapascans.¹⁵ In this case geographical considera-

⁹ Samuel Hearne, *A Journey from Prince of Wales' Fort in Hudson's Bay to the Northern Ocean* (London, 1795), pp. 191-194; E. Petitot, *Traditions indiennes du Canada Nord-Ouest* (Paris, 1886), pp. 434-436.

¹⁰ James Teit, *The Thompson Indians of British Columbia*, Publications of the Jesup North Pacific Expedition, V (1905), 311-321.

¹¹ My own field information.

¹² Kroeber, *The Religion of the Indians of California*, pp. 334 ff.

¹³ Boas, *The Eskimo of Baffin Land and Hudson Bay*, p. 121.

¹⁴ L. H. Morgan, *op. cit.*, p. 187.

¹⁵ Petitot, *op. cit.*, p. 435.

tions point with overwhelming force to an explanation by historical contact. The above-mentioned instance of the new-fire ceremony forms perhaps an almost equally good case in point; but in other cases the matter is less certain, though odd features of capricious distribution haunt the mind with visions of possible historical connection. Thus Boas refers to the rather striking analogies between the tortures of the Kwakiutl War Dance and the Plains Indian Sun Dance.¹⁶ The phenomenon of ceremonial buffoonery that crops up among the Iroquois, the western Ojibwa, many of the Plains tribes and Pueblo Indians, as well as in California and on the Northwest coast, presents probably too general a similarity (except among tribes obviously in contact with one another) to be considered of historical significance. Nevertheless some specific analogies are puzzling. Thus the Tlingit have so distinctive an element of Plains Indian clownishness as the use of "backward speech"; that is, expression of the exact opposite of the intended meaning.¹⁷ Only a much fuller knowledge of the distribution of ceremonial elements and complexes will help us estimate the relative value of the theories of historical contact and independent development in such concrete instances. For the time being, it will be well to regard historical contact as established only in the clearest cases, though these are by no means few (see below, "Diffusion of Ceremonials").

MYTH AND RITUAL

In many cases a ceremony is derived by the natives from a myth accounting for its origin. Native statements, however interesting in themselves, cannot of course be taken as objective historical fact. Hence arises the question, Is the myth the primary phenomenon on which the ceremony is founded, or is it merely a secondary explanation of the origin of a preëxisting ceremony? A considerable amount of information bearing on this problem has been recorded; here only enough can be presented to illustrate essential principles.

The Crows and Blackfeet share a ceremonial planting of Sacred Tobacco. As this performance has not been found among other

¹⁶ Boas, "The Social Organization . . . of the Kwakiutl Indians," pp. 495, 661.

¹⁷ Swanton, "Social Condition . . . of the Tlingit Indians," p. 440.

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tribes of this area, and as there are similarities of detail, the single origin of the common features of the ceremonies as performed by the two tribes is certain. Among the Blackfeet, however, the Sacred Tobacco forms part and parcel of the Beaver Medicine Bundle. This is in its entirety derived from a Beaver, who, after luring away a Blackfoot's wife, indemnified the husband by sending the woman back with the Beaver Bundle.¹⁸ The Crows, on the other hand, do not associate their Tobacco with the beaver, but identify it with the stars. According to the most popular version, the discovery of the Tobacco dates back to the period of their legendary separation from the Hidatsa, when one of two brothers was adopted by the stars, blessed with the vision of the Tobacco, and instructed as to the ceremonial planting. The same ritualistic features are thus associated with two distinct myths in the two tribes; hence at least one of the myths is certainly secondary, which establishes in principle the possibility of such a secondary association. For the secret ceremonials of the Northwest coast of North America, a corresponding conclusion was long ago drawn by Professor Boas. Of the several tribes sharing the ceremonies in question, some derive their performances from the wolves, others from heaven, still others from the cannibal spirit or from a bear. In all cases but one, the explanation *must* be secondary, and, with the possibility of such explanation established, it becomes psychologically justifiable to treat the residual case as falling under the same category: the ritualistic myth is an aetiological myth. Ehrenreich has duly emphasized the occurrence of demonstrably secondary connection between ritual and myth in North America; and, since the rituals and myths of this continent are better known than those of any other area of equal magnitude, he rightly insists that the conclusions derived from this basis have general significance for the problem of the relationship of these associated elements.¹⁹

Boas and Ehrenreich not only strengthen the case for secondary connection, but also demonstrate the workings of the aetiological

¹⁸ Clark Wissler, *Mythology of the Blackfoot Indians*, AMNH, Anthropological Papers, II (1908), 1-164.

¹⁹ Paul Ehrenreich, *Die allgemeine Mythologie und ihre ethnologischen Grundlagen* (Leipzig, 1910), p. 84.

instinct by proving that in not a few cases a ritual is accounted for in a single tribe by attaching it to a folk-tale or folk-tale episode of very wide distribution. In such instances the question of the priority of the tale or ritual is, of course, immaterial: there is secondary association of previously independent units.

Thus, among the Heiltsuk alone, the story of a woman who gave birth to dogs is used to explain the establishment of the Cannibal Society. As this tale is found without any ceremonial associations among the Eskimo, all the northern Athapascans, and all the Northwest coast Indians, its secondary application to the Heiltsuk ritual is manifest. In other words, not only is the same ritual explained by different myths in different tribes, but, in the attempt to account for the origin of the ritual, there is a tendency to use popular tales that come to hand.²⁰ This tendency, it may be noted, is strongly developed in other regions of the continent. The Hidatsa and Mandan associated the custom of planting certain offerings by the bank of the Missouri with the tale of the young man who ate of the flesh of a snake, became transformed into a snake, and was carried to the Missouri by his comrade.²¹ According to my own field data, these offerings formed part of the Hidatsa Missouri River ceremony, one of the sacred rituals of the tribe. Similarly, the Bird ceremonial of the same tribe is connected with the exceedingly widespread story of the thunderbird's antagonism to a water-monster. Examples of this type certainly seem to justify in considerable measure Ehrenreich's conclusion: "Jedenfalls liegen der Regel nach einem Kultmythus schon anderweitig bekannte Stoffe oder in anderen Verbindungen vorkommende mythische Elemente zugrunde. Was das Ritual dem hinzufügt, ist äusseres Beiwerk, als Anpassung zu bestimmtem Zweck."

There are many instances, however, where the connection between ritual and myth is of a more intimate nature. The Black-foot myth of the Beaver Bundle, quoted above, which forms the

²⁰ Boas, "Social Organization . . . of the Kwakiutl Indians," pp. 662-664; *idem*, *The Mythology of the Bella Coola Indians*, p. 126.

²¹ Maximilian, Prinz von Wied-Newied, *Reise in das innere Nord-America* (Coblenz, 1839), II, 184-186, 230-234. The tale, without ritualistic associations, occurs among the Assiniboine, Arapaho, Gros Ventre, Crow, Omaha, and Arikara. See Lowie, *The Assiniboine*, p. 181.

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pattern for a series of other ritualistic myths, may serve as an example. "In most ceremonies," writes Wissler, "the origin of the ritual is regarded as the result of a personal relation between its first owner and its supernatural giver; each ceremony or demonstration of the ritual being a reproduction of this formal transfer."²² This notion is so strongly developed among the Hidatsa that, whenever one of my informants was unable to recount the vision through which knowledge of a particular ceremony was derived, he at once suggested that the ceremony must be of foreign origin. Substantially there is no difference between the origin myths and the accounts by men still living of such visions as explain the institution of recent ceremonies: both recount the meeting with the visitant, his ceremonial gifts, and relevant instructions. The only difference lies in the fact that stories of the first class have already, while those of the second class have not yet, become part of the traditional lore of the tribe, or clan, or society. Again, the secondary character of the myth is at once manifest: no tribe could develop a story explaining ceremonial details (any more than an individual could have a vision of such ritualistic proceedings), unless such ceremonial features already formed part of the tribal consciousness. The myth simply recites the preëxisting ritual, and projects it into the past.

There is, of course, nothing in the nature of human psychology that would prevent myths from being dramatized in ceremony. It is simply an empirical fact that in North America such dramatization, if not wholly absent, is certainly subordinate in importance to the aetiological utilization of the myth. The Midewiwin ceremony does not dramatize the doings of Mänäbush and his brother; but the celebrants recite the story and add to it an account of the origin of their own doings. The Omaha Shell Society interpret the ceremonial shooting practised by members as a dramatic representation of the shooting of four children in the Origin Myth; but, as Radin has shown,²³ the shooting ceremony is so widespread a feature in other tribes that it cannot have originated from this particular tale. The Okipa performers do not enact their tale of a flood, but use that tale as a partial explanation

²² Wissler, *Mythology of the Blackfoot Indians*, p. 13.

²³ Paul Radin, *op. cit.*, p. 182.

of their annual festival. A secondary reflex effect of the myth on the ritual and its symbolism is of course undeniable. Thus in the Okipa we do find an actor impersonating the mythic hero, Nū'mak-máxana; but, while the actor narrates the tale of the flood, he does not, so far as we can judge, perform the actions of his prototype at the time of the flood or on any other occasion. Similarly, among the Hidatsa, the hero-trickster figures in many ceremonial performances; but he does not act out his heroic or clownish exploits.²⁴ Again, among the Bellacoola, the kūsūt ceremonial appears to the native mind as a dramatic representation of legendary happenings. As a matter of fact, we do meet with impersonations of the deities of the Bellacoola pantheon; but the essential elements of the ceremonial, such as the cannibalistic practices, have an origin, not in the highly specialized Bellacoola mythology, but in actual observances shared in recent times by a number of Northwest coast tribes, and connected in part with war customs.

So among the Hopi the episodes of the legends associated with ceremonials do not determine at all definitely the sequence of ceremonial procedure; here also the ritual appears as a less variable and as a preëxisting feature.²⁵ Finally may be mentioned the Mohave case. Here the ceremonies not connected with mourning "consist essentially of long series of songs, occupying one or more nights in the recital, which recount, in part directly but more often by allusion, an important myth. At times the myth is actually related in the intervals between the songs. In some cases, dancing by men or women accompanies the singing; but this is never spectacular, and in many cases is entirely lacking."²⁶ But, though the prominence of the myth is here so great that the ceremonies in question are only ceremonial recitations of myths, this very fact obviously precludes dramatization of the mythic incidents.

DIFFUSION OF CEREMONIALS

In the Plains area, the diffusion of ceremonies is in some cases not merely a plausible hypothesis, but an historical fact. No one

²⁴ G. H. Pepper and G. L. Wilson, "An Hidatsa Shrine and the Beliefs Respecting It," AMNH, Memoirs, II (1908), 320.

²⁵ Fewkes, "The Group . . . Called Katcinas," pp. 253 ff.

²⁶ Kroeber, *The Religion of the Indians of California*, p. 340.

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could doubt that the Hot Dance of the Arikara, Ruptare Mandan, and Hidatsa (involving in each instance the plunging of the performers' arms into scalding hot water) must have been derived from a common source. But we have in addition Maximilian's assurance that the ceremony was obtained by the Hidatsa from the Arikara.²⁷ Lewis and Clark (1804) mention ceremonial foolhardiness as a feature borrowed by the Dakota from the Crows.²⁸ Within the memory of middle-aged men at least, two ceremonies have been introduced into the northern Plains from the south. The peyote cult, which is found among the Tepehuane, Huichol, and Tarahumare of Mexico, flourishes among the Kiowa and Comanche, and has thence traveled northward to the Arapaho, and even to the Winnebago.²⁹ The Grass Dance was introduced among the Crows by the Hidatsa about 1878; among the Blackfeet by the Gros Ventre, about 1883; among the Flathead by the Piegan, in quite recent times.³⁰ It seems to have originated among the Omaha and cognate tribes, including the Ponca, Osage, Iowa, and Oto.³¹ In addition to the tribes already mentioned, its occurrence has been noted among the Pawnee, Dakota, and Assiniboin. Other unexceptionable instances are numerous. Thus a Medicine Pipe Dance of the Pawnee *hako* type was adopted by the Crows from the Hidatsa during the second half of the nineteenth century; and the Hidatsa remember that their Medicine Pipe ceremony was in turn derived from the Arikara. A sacred Horse Dance practised by the River Crows was secured from the Assiniboin. The same division of the Crows adopted a Crazy Dog Society from the Hidatsa about thirty-five years ago. To pass to another area, the Kwakiutl proper ascribe the origin of their cannibalistic ceremonial to the Heiltsuk, from whom they derived the practice in approximately 1835; while the Tsimshian derive

²⁷ Maximilian, *op. cit.*, II, 144.

²⁸ Lewis and Clark, *Original Journals of the Lewis and Clark Expedition* (Thwaites ed.; New York, 1904-1905), p. 340.

²⁹ Kroeber, *The Arapaho*, p. 320; *idem*, *Handbook of the Indians of California*, BAE, Bull. 78 (1925); Paul Radin, "A Sketch of the Peyote Cult of the Winnebago," *Journal of Religious Psychology*, III (1914), 1-22.

³⁰ Lowie, *Societies of the Crow, Hidatsa and Mandan Indians*, p. 200; Wissler, *Societies and Dance Associations of the Blackfoot Indians*, p. 451.

³¹ A. C. Fletcher and F. La Flesche, "The Omaha Tribe," BAE, 27th Ann. Rept. (1911), p. 459.

a corresponding custom from the same source, whence it reached them probably ten years before.³² While native tradition is often untrustworthy, the date set by it in these instances is so recent that scepticism is hardly in place. This is especially true, since linguistic evidence supports the account of the Indians; for practically all the names applied to the Tsimshian performances are derived from the Kwakiutl, and the characteristic cry of the cannibal is likewise a Kwakiutl word.³³

The foregoing instances, which could be considerably multiplied, illustrate diffusion as an observed or recollected historical phenomenon. Even in the absence of such direct evidence, however, the theory of diffusion is in many cases inevitable. Among the graded ceremonies of the Gros Ventre, the lowest is a Fly Dance, which is said to have been instituted by a Mosquito; the members imitated mosquitoes, pursuing people and pricking them with spines and claws. The lowest of the graded Blackfeet ceremonies recorded by Maximilian in the early thirties of the nineteenth century was likewise practised by a Mosquito Society, whose members imitated mosquitoes, maltreating their fellow-tribesmen with eagle-claw wristlets.³⁴ The coincidence is so complete in this instance, that a common origin is certain, especially since the Blackfeet and Gros Ventre have been in intimate contact with each other, and since the only other people known to have had a Mosquito ceremony, the Sarsi, have also been closely associated with the Blackfeet. In the case at hand, we are even able to go a step farther, and ascertain not merely the fact, but the direction, of the diffusion process. The Gros Ventre are linguistically most closely allied with the Arapaho, with whom they once lived, and whose ceremonial system presents striking resemblances to their own. The presence of a Mosquito Dance among the Gros Ventre constitutes one of the glaring disparities amidst otherwise far-reaching likenesses: we may therefore reasonably infer that the difference resulted from the adoption of the Blackfeet Mosquito Dance by the Gros Ventre subsequent to their separation from the Arapaho.

³² Boas, "The Social Organization . . . of the Kwakiutl Indians," p. 664.

³³ *Ibid.*, p. 652.

³⁴ Lowie, *The Assiniboine*, p. 82.

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In other cases we must be content to infer the mere fact of diffusion from the observed homologies. For example, the Arapaho and Cheyenne have each a Dog organization with four scarf-wearing officers pledged to bravery, and characterized by the same ceremonial regalia, such as dew-claw rattles, feather head-dresses, and eagle-bone whistles. The union of these logically quite unrelated features in adjoining tribes establishes beyond doubt a common origin; but I am not acquainted with any specific data that would indicate whether the Arapaho borrowed from the Cheyenne, or vice versa. Cases of this type are exceedingly common in every one of the principal culture areas; and where similarities extend beyond the confines of these conventional provinces, or beyond a linguistic stock that more or less coincides with a cultural group, the fact of transmission is emphasized by the type of distribution found. Thus the shooting of a magical object with intent to stun candidates for initiation into the Midewiwin Society occurs among the Central Algonkian. In one form or another, this shooting is also a feature of societies among several Siouan tribes; but these are precisely those tribes which have been in close contact with the Central Algonkian—the eastern Dakota, southern Siouan, and Winnebago. The Sun Dance offers another case in point. This ceremony is found among the majority of Plains tribes, but has also been celebrated by several divisions of the Shoshonean stock, who properly belong, not to the Plains, but to the Plateau area. Here, again, the type of distribution is such as might be expected on the theory of diffusion: of the Shoshoni proper, the Lemhi did not practise the Sun Dance, but it is still performed at Wind River and Fort Hall, where the Shoshoni come more in contact with Plains peoples.

The fact of diffusion must, then, be regarded as established; and the very great extent to which ceremonials have traveled from tribe to tribe, coupled with undoubted diffusion of other cultural elements in North America, indicates that, while the process has been greatly accelerated by improved methods of transportation and other circumstances promoting intertribal intercourse, it must have been active prior to these modern conditions due to white influence.

The next problem is: How have ceremonial features been dif-

fused? Plausible answers to this question seem relatively easy. Ceremonial regalia were often carried in war, and might readily be imitated, or snatched away from the enemy, and thus become a ceremonial feature of a new tribe. Among the Kwakiutl and their cognates, alien dance regalia were often secured by killing the owner.³⁵ During meetings of friendly tribes, dances were sometimes performed for the entertainment of the visitors, who might thus learn a new ceremony. It was in this way that the River Crows came to have their Muddy Mouth performance.³⁶ Wherever a ceremony was considered (as frequently happened) a form of property, the right to perform it was naturally transferable to an alien who paid the customary amount of goods. Thus the Hidatsa secured the Hot Dance from the Arikara by purchase.

Before going further, we must be clear as to what is really transmitted through the agencies suggested. For example, the method of acquiring certain regalia through killing the owner does not account for the diffusion of the ceremony itself which these regalia symbolize. Take an instance cited by Boas. The Matilpe had not been permitted by the other tribes to acquire the Cannibal performer's regalia. At one time their village was approached by a party of men and women from the northern tribes, one of the men wearing the badge of the Cannibal order. Two Matilpe youths killed the strangers, and one of them assumed the Cannibal's cedar-bark ornaments, and at once began to utter the characteristic Cannibal cry, "for now he had the right to use the dance owned by the man whom he had killed." It is clear that the knowledge of the performance preceded the acquisition of the badge. In the native mind, to be sure, the Cannibal Dance was a form of property that could be acquired by killing the owner; and before its acquisition it did not, from the native point of view, form part of the Matilpe culture. But in reality, of course, it did form part of that culture; for otherwise the attitude of the Matilpe, both before and after the murder, would be impossible. The essential problem involved is not how the Matilpe secured the symbols of the ceremony (however important these may ap-

³⁵ Boas, "The Social Organization . . . of the Kwakiutl Indians," pp. 424-431.

³⁶ Lowie, *Societies of the Crow . . . Indians*, pp. 197 ff.

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pear to the native mind), but how the Matilpe came to participate in the knowledge of the ceremonial. The murder did not effect simple bodily introduction of a new ceremony, but only bodily introduction of new ceremonial badges, which were fitted into their customary ceremonial associations through prior knowledge of the ceremonial complex to which they belong.

It is, however, quite intelligible how such knowledge spread to the Matilpe through simple attendance as onlookers at performances of other tribes, for in that capacity they were hardly in a different position from the uninitiated spectators who belonged to the tribe of the performers. Whether an observed ceremonial routine is actually imitated (as in the case of the Muddy Mouth Dance of the River Crows), or remains unexecuted, contingent on fulfilment of requirements due to existing property concepts, is, from the point of view of diffusion, relatively unimportant. The point is that not only tangible articles, but even an objective series of acts, songs, etc., may readily spread from tribe to tribe. In Australia it has been proved that ceremonies travel in various directions, like articles of exchange, and that frequently "a tribe will learn and sing by rote whole corroborees in a language absolutely remote from its own, and not one word of which the audience or performers can understand the meaning of."³⁷ Illustrations of similar forms of borrowing are not lacking in North America. Thus the Winnebago chant Sauk songs during their Medicine Dance; and the music of songs is readily passed on from tribe to tribe, as in the case of the Grass Dance.

When there is esoteric ceremonial knowledge, the process of transmission implies, of course, far more intimate contact. Here the borrowing individuals or groups must be treated, for purposes of initiation, as though they belonged to the tribe from which the knowledge is obtained. The Arikara trick of plunging one's arm into scalding hot water without injury could not be imitated by the Hidatsa on the basis of mere observation; instruction must be *bought*, as it would be bought by an Arikara novice from an Arikara adept. Through similarly close personal contact, the

³⁷ W. E. Roth, *Ethnological Studies among the North-West-Central Queensland Aborigines* (Brisbane, 1897), p. 117.

Medicine Pipe ceremony spread from individual Arikara to individual Hidatsa, and from individual Hidatsa to individual Crows.

To sum up: transmission of external features, such as ceremonial paraphernalia, is possible on the basis of superficial, possibly even hostile, meetings; friendly intertribal gatherings render possible the borrowing of ceremonial routine, songs, and the like, in short, of the exoteric phases of the complex; while initiation into the inner meaning of a ceremony becomes feasible only through the closest form of personal contact.

Nevertheless the problem of diffusion is still far from being exhausted. Even where a ceremony seems to be bodily transferred, it may become different because of the differences in culture between the borrowing and transmitting tribes; that is to say, even an entire ceremony is not an isolated unit within the culture of the tribe performing it, but has definite relations to other ceremonies and to the tribal culture generally. Even tribes sharing in large measure the same mode of life tend to diverge as regards specific conceptions of social and ceremonial procedure. The "same" ceremony may thus enter different associations, and in so far forth become different through its novel relations. There can be no doubt that the Tlingit and Haida potlatches represent a single cultural phenomenon. Nevertheless there is a remarkable disparity between the associations of the great potlatches of these tribes. Among the Haida, the main festival was conducted by a chief in behalf of his own moiety, and was intended only to enhance his social standing. The Tlingit performed a potlatch for the benefit of the complementary moiety and for the sole avowed purpose of showing respect for the dead.³⁸ This illustration is instructive, because it embodies both types of changes that a transmitted ceremony undergoes,—a change in objective relations, which, however, cannot in many instances fail to affect the subjective attitude of the performers or borrowing tribe at large; and a change of the ostensible object, of the theoretical *raison d'être*, of the performance. These types of changes had best be considered separately. I shall approach the primarily objective

³⁸ Swanton, "Social Condition . . . of the Tlingit Indians," pp. 434 ff.; *idem*, *Contribution to the Ethnology of the Haida*, pp. 155 ff., 162.

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alterations undergone by a borrowed ceremony through a consideration of the specific tribal patterns for ceremonial activity; and I shall consider the changes of avowed *raison d'être* in diffused ceremonies in the section dealing in a general way with the ends sought through ceremonial performances.

To avoid misunderstanding, it must be noted that by no means all changes of diffused ceremonies can be brought under these two heads. This is best seen when comparing the established variations in the performance of the same ceremony by local subdivisions of the same tribe. Thus we find that in some Haida towns the Grizzly Bear spirit inspired only women, while in others there was no such restriction.³⁹ The River Crows adopted the Crazy Dog Dance from the Hidatsa without assimilating it to the old Crow dances, while the Mountain Crows at once assimilated it to the rivalry concept of their Fox and Lumpwood organizations.⁴⁰ The unique historical conditions upon which such changes of borrowed ceremonies depend are not different in type from those which determine modifications in an indigenous ceremony, and are in neither case amenable to generalized treatment.

CEREMONIAL PATTERNS

Among the Arapaho the seven ceremonies distinctive of the age-societies, as well as the Sun Dance, are performed only as the result of a pledge made to avert danger or death.⁴¹ The dances of the Kwakiutl, differing in other respects, resemble one another in the turns about the fireplace made by entering dancers; paraphernalia of essentially similar type (head-rings, neck-rings, masks, whistles) figure in Kwakiutl performances otherwise distinct; and the object of apparently every Kwakiutl society's winter ceremonial is "to bring back the youth who is supposed to stay with the supernatural being who is the protector of his society, and then, when he has returned in a state of ecstasy, to exorcise the spirit which possesses him and to restore him from his holy madness."⁴² Among the Hidatsa the right to each of a considerable number of esoteric rituals must be bought from one's father: in

³⁹ Swanton, *Contribution to the Ethnology of the Haida*, p. 171.

⁴⁰ Lowie, *Societies of the Crow . . . Indians*, p. 148.

⁴¹ Kroeber, *The Arapaho*, pp. 158, 196.

⁴² Boas, "The Social Organization . . . of the Kwakiutl Indians," pp. 43 ff.

each case the requisite ritualistic articles were supplied by a clansman of the buyer's father; a "singer" conducted the ceremonies; the purchaser received the ceremonial bundle, not directly, but through his wife; and so forth.⁴³ All important bundle ceremonies of the Blackfeet require a sweat-lodge performance; in nearly all rituals the songs are sung by sevens; for almost every bundle some vegetable is burned on a special altar; and every ritual consists essentially of a narrative of its origin, one or more songs, the opening of the bundle, and dancing, praying, and singing over its contents.⁴⁴

It would be manifestly absurd to assume that the notion of performing ceremonies to ward off death originated eight times independently among the Arapaho; that the originators of the Kwakiutl Cannibal ceremonial and the originators of the Kwakiutl Ghost Dance independently conceived the notion of wearing neck-rings;⁴⁵ and so forth. Wissler has forcibly brought out the point that among the Blackfeet the Beaver Bundle owners seem to have established a pattern of ceremonial routine that has been copied by the owners of other bundles; and many additional illustrations could be cited to prove that, in every tribe with a highly developed ceremonial system, a corresponding pattern has developed. The psychology of this development has been felicitously compared by Goldenweiser with the process of borrowing ideas from an alien tribe: in both cases a novel idea is suggested, and may be rejected, or partly or wholly assimilated.⁴⁶ Whenever such an idea is generally adopted within a tribe, it tends to assume the character of a norm that determines and restricts subsequent thought and conduct. The Plains Indian generally ascribes any unusual achievement, not to personal merit, but to the blessing of a supernatural visitant; hence he interprets the invention of the phonograph in accordance with this norm. Among the Hidatsa it is customary to give presents to a father's clansman; hence an Hidatsa

⁴³ My own field notes.

⁴⁴ Wissler, *Ceremonial Bundles*, pp. 257, 271, 254, 101, 251.

⁴⁵ Boas, "The Social Organization . . . of the Kwakiutl Indians," in which cf. figs. 81, 147.

⁴⁶ A. A. Goldenweiser, "The Principle of Limited Possibilities in the Development of Culture," *Journal of American Folk-Lore*, XXVI (1913), 287.

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purchasing admission into an age-society selected from among the group of sellers a member of his father's clan. The notion at the bottom of the norm originates, of course, not as the notion of a norm, but like all other thoughts that arise in individual consciousness; its adoption by other members of the social group is what creates the pattern. We cannot, without tautology, generalize as to the type of concept that will become a model; indeed, we have found that, in two different bands of the same tribe, an already established concept may in the one case assimilate an alien introduction, and in the other capriciously fail to exert any influence on it. All that we can say is, that patterns exist, and are one of the most active forces in shaping specific cultures.

From the point of view here assumed, a problem that might otherwise arise in the study of North American ceremonialism, and has already been touched upon, assumes a somewhat different aspect. Finding a very complex ceremonial system in certain parts of the continent, in the absence of such a system in others we might be tempted to ascribe the difference to a psychological difference between the respective tribes. In some measure, to be sure, extensive diffusion of cultural elements in some areas as compared with others would account for the observed phenomenon. If at one time the tribes of the Northwest coast or the Plains, taken singly, possessed a ceremonial culture as simple as that of California or the Plateaus, but spread their respective ceremonials among other tribes of the same area whose ceremonials they in turn adopted, then complexity might ensue without any cause other than conditions favorable for cultural dissemination. On the other hand, the purely internal action of the pattern principle would suffice to produce a corresponding complexity. The Crows have a Tobacco order composed in recent decades of perhaps a dozen or more distinct branches or societies, all sharing the right to plant sacred tobacco, and differing only in the specific regalia, and instructions imparted to the founders in the visions of other experiences from which the branches are derived. Visions of similar type are not lacking among such a tribe as the Shoshoni; but in the absence of an integrating pattern they have not become assimilated to a ceremonial norm. A Crow who

belonged to the Tobacco order, and stumbled across a nest of curiously shaped eggs, would form an Egg chapter of the Tobacco order; a Shoshoni might experience precisely the same thrill under like conditions, but the same psychological experience could not possibly result in the same cultural epiphenomenon. The several Tobacco societies of the Crow do not represent so many original ideas, but are merely variations of the same theme. There is, then, only one basic idea that the Crow have and the Shoshoni have not,—the idea of an *organization* exercising certain ceremonial prerogatives, for the ceremonial features in themselves are of a type probably not foreign to any North American group. The complexity of the socio-ceremonial life of the Crows is thus an illusion due to the fact that this single idea became a pattern.

The pattern principle is also of the greatest value in illuminating the precise happenings during the process of diffusion. It has been shown in another section, that a borrowed ceremony, even when bodily adopted, becomes different, because it originally bore definite relations to other cultural features of the transmitting tribe; and, unless these additional features happen to exist in the borrowing group, the same unit must assume a different cultural fringe. What happens in many, perhaps in the majority of, such cases, is that the borrowed elements are fitted into conformity with the pattern of the borrowing tribe. Thus the Dog Society of the Crows is traced back to the Hidatsa. But among the Hidatsa this ceremonial body is one of a graded series of military societies in which it occupies a definite position; and entrance into it, as in the case of the rest, is a matter of purchase. Since the Crows neither grade their military organizations nor exact an entrance fee in any of them, the Dog Society naturally lost the impress of the Hidatsa mold so far as these features were concerned. Moreover, it was made over to fit the Crow scheme. Entrance into the society was, as for all other Crow military societies, either a matter of choice, or, more commonly, was stimulated by the desire of members to have the place of a deceased member filled by a relative. Again, while police duties among the Hidatsa were the exclusive right of the Blackmouth Society, the Crow organizations all took turns at exercising this social function, the Dog Society among the rest. Thus the Dog

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Society with all its ceremonial correlates came to enter quite new combinations and to assume a specifically Crow aspect.⁴⁷

To Radin we are indebted for a suggestive investigation of the mechanism of ceremonial borrowing with special reference to the selective and assimilative influences exerted by the recipient culture on the borrowed features. The peyote cult, a very recent importation from Oklahoma, has rapidly risen to a most important position in the life of the Nebraska Winnebago. A detailed study indicates that the only really new thing introduced was the peyote itself, its ceremonial eating, and its effects. Several Christian elements that enter into the present Winnebago performance prove to be similar to preëxisting aboriginal concepts, so as to suggest that their acceptance was due to this conformity. The founder of the Winnebago cult seems to have at once placed the new plant in the category of medicinal herbs, and accordingly to have associated with it the traditional shamanistic ideas. The organization of the new society automatically conformed to the Winnebago norm. The origin narrative developed by one of the converts "assumed all the characteristics of a Winnebago fasting experience and ritualistic myth, similar to those connected with the founders of the old Winnebago cult societies. In its totality, the atmosphere of the peyote cult became thus highly charged with the old Winnebago background."⁴⁸

THE OBJECT OF CEREMONIES

Speaking of the Mandan Okipa, Catlin recognizes three "distinct and ostensible objects for which it was held": it was an annual commemoration of the subsidence of the deluge; it was an occasion for the performance of the Bull Dance, which caused the coming of buffalo herds; and it was conducted in order to inure young men to physical hardship, and enable the spectators to judge of their hardihood.⁴⁹ The diversity of these alleged objects suffices of itself to suggest that the Okipa is a *complex performance*; that it would be vain to try to account for its ori-

⁴⁷ Lowie, "Some Problems in the Ethnology of the Crow and Village Indians," *American Anthropologist*, XIV (1912), 70; *idem*, *Societies of the Crow . . . Indians*, p. 155.

⁴⁸ Radin, "A Sketch of the Peyote Cult of the Winnebago."

⁴⁹ George Catlin, *O-kee-pa* (London, 1867), p. 9.

gin by a simple psychological explanation. It is a priori psychologically conceivable that the Okipa (that is, an annual four-day summer festival) originated as a celebration commemorative of the mythical flood, however improbable this may appear from our considerations of "Myth and Ritual"; but, if so, the conception that it was intended to attract the buffalo and the conception that it was an ordeal for the young men were secondary. Or we may assume that the ordeal concept was primary; then the two other alleged functions were secondary. And a corresponding conclusion seems inevitable if we suppose that the enticing of the buffalo was the original motive for the festival. In a more acceptable form, this theory might be stated as assuming that three originally independent ceremonies performed for diverse ends somehow became welded together into what then became the Okipa.

Before going further, it will be well to demonstrate that the complexity of the ceremony is an historical fact. This becomes at once obvious when we consider the distribution of two of our three hypothetical elements. The buffalo-calling ceremony is by no means a peculiarity of the Mandan Okipa, but a ceremony very widely diffused over the Plains area: indeed, a buffalo-calling ceremony not differing in principle from that of the Okipa was performed by the Mandan themselves independently of the Okipa;⁵⁰ and a ceremony undertaken for the same ostensible purpose and with corresponding mimetic features was practised by the Mandan White Buffalo Cow Society.⁵¹ What is true of the buffalo-calling feature applies with even greater force to the voluntary self-torture element. This appears with all its characteristic details—such as piercing of the breasts, insertion of skewers, suspension from a pole, and dragging of buffalo-skulls—not only in the Sun Dance of various tribes (where there is a collective torture strictly comparable to that of the Okipa), but also among the Dakota, Crows, and other Plains peoples, as a fairly normal procedure in the individual quest for supernatural aid.⁵² That the buffalo-calling ceremony and the specific self-torturing practices

⁵⁰ Maximilian, *op. cit.*, II, 181, 264 ff.

⁵¹ Lowie, *Societies of the Crow . . . Indians*, pp. 346–354.

⁵² J. O. Dorsey, *op. cit.*, pp. 436 ff.

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under discussion were at one time independent of each other, and of whatever other features they are combined with in the Okipa, must be considered an established fact: indeed, the complexity is greater than the theory here discussed would indicate. To mention but one conspicuous feature, a great deal of time is consumed in the Okipa with dances by mummers impersonating animals and closely mimicking their appearance and actions. The performances are objectively, in a rough way, comparable to the Bull Dance, but have nothing to do with any solicitude for the food supply, since many of the beings represented are not game animals. These animal dances rather suggest the dream-cult celebrations of the Dakota, especially as the performers chanted sacred songs distinctive of their parts, and taught only on initiation and payment of heavy fees.⁵³ The mimetic animal dance thus forms an additional element of the Okipa complex.

The complex character of the ceremony is thus an historical fact. How, then, shall we interpret the equally certain fact that, to the native consciousness, it appeared as a unified performance instituted by the mythical hero Nū'mak-máxana,⁵⁴ and celebrated, if not for the specific reasons assigned by Catlin, from the vaguer motive of promoting the tribal welfare in general?⁵⁵

We shall not go far wrong in putting the alleged *raison d'être* of the Okipa in the same psychological category with ritualistic myths. As the myth is an aetiological afterthought associated with a preëxisting rite, so the alleged object of a complex ceremony may be merely an afterthought engrafted on a preëxisting aggregation of ceremonial elements. In the one case it is the aetiological, in the other the teleological, feature that welds together disparate units, and creates the illusion of a synthesized articulated whole. If the hero Nū'mak-máxana ordered the Mandan to practise a particular combination of un-unified observances, these performances become unified by that mythical fiat; and the causal requirements of the native, at the stage when rationalization sets in, are satisfied. At this stage the teleological point of view naturally serves the same purpose: in practice, in

⁵³ Catlin, *op. cit.*, pp. 19 ff.; Maximilian, *op. cit.*, II, 178.

⁵⁴ Maximilian, *op. cit.*, II, 172.

⁵⁵ E. S. Curtis, *The North American Indian* (Cambridge, 1907-1930), V, 26.

fact, it largely coincides with the aetiological attitude. If Nū'mak-máxana instituted the annual festival, he did so for the purpose of benefiting the Mandan, and dereliction would spell tribal disaster. On the other hand, if the ceremony insures the common-weal, no further cause for its performance is required.

The principle here illustrated by the Okipa may be demonstrated in even more satisfactory fashion for the Sun Dance of the Plains tribes. Whatever may be the avowed purpose of this performance, certain elements are practically uniform throughout the area; for example, the selection and felling of a tree treated as an enemy, the erection of a preparatory and a main lodge, and a several-days' fast culminating (except among the Kiowa) in torture proceedings of the Okipa type. The Sun Dance of the Crows was performed exclusively in order to secure vengeance for the slaying of a tribesman; among the western Algonquian tribes it was vowed in the hope of delivering the pledger or his family from sickness or danger; while benefits of a vaguer and more public character were expected by the western Dakota, Hidatsa, and Kiowa.⁵⁶ In view of this diversity of ends sought, we cannot associate the ceremonial routine defined above with *any* of the ostensible objects of the Sun Dance; for in all cases but one the object *must* be secondary, and, from an argument analogous to that used in the consideration of "Myth and Ritual," the residual case appears amenable to the same psychological interpretation. In other words, the ostensible motive of complex ceremonies is not the genuine or original motive, but embodies merely the present native *theory* of the reason for the performance.

Several questions naturally arise: If we cannot directly interpret a complex ceremony, can we not at least give a psychological interpretation of its components? further, if we can resolve it into such constituents, how must we conceive the process by which originally unrelated elements became joined together (as we have assumed) through historical accident, to be integrated only at a later stage by some rationalistic synthesis? and, finally,

⁵⁶ G. A. Dorsey, *The Arapaho Sun Dance*, pp. 5 ff.; *idem*, *The Cheyenne*, p. 58; Walter McClintock, *The Old North Trail* (London, 1810), p. 170; Kroeber, *Ethnology of the Gros Ventre*, p. 251; H. L. Scott, "Notes on the Kado, or Sun Dance of the Kiowa," *American Anthropologist*, XIII (1911), 347; J. O. Dorsey, *op. cit.*, p. 451.

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if the native theory is merely an interesting speculative misinterpretation of native psychology, what is the present psychological correlate of those complicated series of observances under discussion?

Let us consider first of all the second question. Analysis resolves a ceremony into a number of disparate elements; how did these ever become joined together? We are here confronted by the problem of secondary association, a large topic to which only a few words can be devoted in this article. In the first place, we should beware of confounding logical with historical analysis. Two features may be not only logically as distinct as musical pitch and timbre, but also as inseparable in reality. This principle has already been expressed by Dr. Radin, though his illustration rather shows how apparently unrelated concepts are nevertheless logically related in the native mind. The notion of a society derived from a water-spirit and the notion of curing disease are apparently distinct; but, if the water-spirit is always associated with the granting of medical knowledge, a vision of the water-spirit and the acquisition of medical skill coincide. Thus, whatever may be the development of the conception entertained regarding the water-spirit, the association between the idea of a society based on a supernatural communication by that spirit and the idea of doctoring is primary.⁵⁷ Here the initial disparity of the elements found in combination proves to be apparent, being merely due to our ignorance of the *tertium quid*. A primary *ceremonial*⁵⁸ association of genuinely distinct and ceremonially indifferent objects may be achieved through their juxtaposition in a vision, as illustrated by many medicine bundles. Thus, a jackrabbit-skin and a bunch of eagle-feathers may together form an ultimate unit of ceremonial stock-in-trade.

Let us now turn to cases of association of elements once existing apart. One cause of secondary association has already been touched upon. Wherever a particular ceremonial concept becomes the predominant one, it tends to assimilate all sorts of

⁵⁷ Radin, "The Ritual . . . of the Winnebago Medicine Dance," pp. 193, 196. The point seems to me to be closely related to that repeatedly made by Lévi-Bruhl in his *Les Fonctions mentales dans les sociétés inférieures* (Paris, 1922), with reference to "participation."

⁵⁸ Otherwise, of course, the association is secondary.

other concepts originally independent of it: thus, in the Crow example of the Tobacco societies and in the case of the Blackfeet Beaver Bundle, which has not only become the pattern for other bundles, but has even absorbed such rituals as the Sun Dance and Tobacco ceremony.⁵⁹ Among the Crows, individual visions by members of the Tobacco order have led to the association of quite heterogeneous features. A Tobacco member who chanced upon curiously-shaped eggs would found an Egg chapter of the order, and initiate new members into it, thus bringing about a connection between egg medicine and the sacred Tobacco; and in corresponding fashion have developed the Weasel, Otter, Strawberry, and other divisions.

In these cases it would seem that the notion of sacredness or ceremonialism is so strongly associated with a particular content that has become the ceremonial pattern that any new experience of corresponding character is not merely brought under the same category as the pattern, but becomes an illustration, an adjunct of the pattern concept. In many other instances, a ceremony may bring about conditions normally associated with certain activities in no way connected with the ceremony itself; and, when these conditions arise in the course of the ceremony, they act as a cue to the performance of the normally associated activities. There is no connection between initiation into a society privileged to plant tobacco for the tribal welfare and the recounting of an individual's war-record; nevertheless, in the Crow Tobacco adoption, the entrance into the adoption lodge is uniformly followed by such a recital. The reason is fairly clear. At every festive gathering of the Crows there is a recital of war-deeds; the Tobacco initiation produces such a gathering, which elicits the customary concomitant; and thus the coup-recital becomes a feature of the Tobacco adoption ceremony. Similarly, every Iroquois festival seems to have been preceded by a general confession of sins.⁶⁰ Still another way by which heterogeneous ceremonial activities or features become associated is, of course, by purchase. The Hidatsa Stone-Hammer Society, according to Maximilian, bought the Hot Dance from the Arikara. But the Stone-Hammers had a

⁵⁹ Wissler, *Ceremonial Bundles*, p. 220.

⁶⁰ L. H. Morgan, *op. cit.*, p. 187.

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ceremony of their own prior to the purchase, which was thus associated with the newly acquired fire-dance and the plunging of arms into hot water.

These few suggestions must suffice to indicate how disparate elements may become secondarily associated.

So far as the interpretation of the single elements is concerned, there is relatively little difficulty. Though we may not be able to comprehend the ultimate origin of a certain mode of ceremonial behavior, we can generally apperceive it as typical of a certain tribe or a certain group of tribes. The fact that the Plains Indians went to fast in a lonely place, looking for a supernatural revelation, may remain an irreducible datum; but, when we disengage from the Crow Sun Dance complex the attempt to secure a vision that is given as its ultimate motive, we at once bring it under the familiar heading of "vision-quest." So we may not know how "four" came to be the mystic number of many tribes; but it is intelligible that, where it is the mystic number, dances, songs, processions, and what not, should figure in sets of four. Prayers, dances, sleight-of-hand performances, the practice of sympathetic or imitative magic, etc., are likewise ultimate facts; but their special forms in ceremonies of which they are part are readily classified with corresponding psychological manifestations.

But the social setting of the cultural elements enumerated during a ceremony cannot fail to lend them a color they otherwise lack. The pledger of the Crow Sun Dance, who sets in motion the tremendous machinery required for the communal undertaking, and is thenceforth subjected to tribal scrutiny, cannot be supposed to be in the same psychological condition as if he were merely seeking a vision in the seclusion of a four-night vigil on a mountaintop. What we find in any complex performance of this type, then, is a number of distinct acts with distinct psychological correlates, integrated, not by any rational bond, but by the ceremonial atmosphere that colors them all.

From this point of view the question "What may be the object or psychological foundation of a ceremony?" becomes meaningless. The psychological attitude is not uniform for the performers of a ceremony: it is not the same for the Sun Dance pledger (who

wishes to compass an enemy's death) and the self-torturing vision-seekers in quest of martial glory. Much less is it the same for the pledger and the self-advertising reciters and enactors of war-exploits or the philandering couples hauling the lodge-poles. But is not the attitude of the pledger the essential thing? to assume this customary view is the surest way to miss the nature of ceremonialism. A Crow Sun Dance pledger wishes to effect the death of an enemy; a Cheyenne Sun Dance pledger wishes to insure the recovery of a sick relative. Why must both have, say, a dramatic onslaught on a tree symbolizing an enemy? From the rationalistic point of view here criticized, the answer is not obvious. It would be in perfect accord with the Plains Indian mode of action for the Crow and Cheyenne simply to retire into solitude and secure a vision bringing about the desired result. If they are not content with this, and require an elaborate ceremonial procedure, that procedure must have an additional *raison d'être*. The absence of intelligible object (from the *native* rationalistic point of view no less than from our own) in a ceremonial feature becomes at once clear, if we regard its very performance as self-sufficient, as gratifying certain specific non-utilitarian demands of the community. View it not as primitive religion, or as a primitive attempt to coerce the forces of nature, but as a free show, and the mystification ceases: ceremonialism is recognized as existing for ceremonialism's sake.

Edward B. Tylor

EDWARD B. TYLOR, WHO DIED ON JANUARY 2, 1917, AT THE age of eighty-four, had long been an historic personality. He loomed up as one of the very last figures rooted in the heroic age of nineteenth-century science, as the peer and comrade in arms of Wallace, Huxley, and Spencer. The dean of ethnologists for two score years, he represented his science before students of other branches of knowledge and, thanks to the high literary quality of his style, before the cultured laity as well. He was read and cited by psychologist and historian, biologist and philosopher, by everyone interested in the ways and thoughts of primitive man. And while the circle of his influence widened, he retained the profound and growing respect of his professional colleagues. Even with the irreverent group of American field workers who turn up their noses at the classical school of ethnologists his prestige remains undiminished and their allegiance is of the kind he himself advocated,—no slavish acceptance of tenets but a following of methods “through better evidence to higher ends.”

Edward Burnett Tylor was born at Camberwell on October 2, 1832, and educated at Grove House School, Tottenham. After a brief business career he traveled for several years and in 1856

American Anthropologist, XIX (April–June, 1917), 262–268.

visited Mexico in the company of Henry Christy, an anthropologist to whose personal stimulation he pays a generous tribute in the second edition of the *Researches*. The American trip led to Tylor's first publication, a book on *Anahuac; or Mexico and the Mexicans* (1861). Several years later appeared the *Researches into the Early History of Mankind and the Development of Civilization* (1865). This work laid the foundation of his professional fame, which reached its acme in 1871 with the publication of *Primitive Culture: Researches into the Development of Mythology, Philosophy, Religion, Language, Art, and Custom*. In 1881 he wrote a most serviceable textbook on *Anthropology: An Introduction to the Study of Man and Civilization*.

Though not a university graduate, Tylor became connected with Oxford, both in the capacity of keeper of the University Museum and as a lecturer, being "reader in anthropology" from 1884 to 1895 and "professor" from 1895 to 1909, when he became an emeritus. Of the numerous honors conferred on him only two need be mentioned here. He was elected to a fellowship by the Royal Society in 1871 and knighted in 1912. A volume of *Anthropological Essays Presented to Edward Burnett Tylor in Honor of His Seventy-fifth Birthday* bore testimony to the regard of his fellow-workers. The bibliography concluding that volume indicates the extraordinary number of smaller and scattered contributions that fell from his pen in the course of years, and we learn with deep regret that a great work he had been preparing for many years was never published, which was also the fate of his ten Gifford lectures on Natural Religion, delivered at Aberdeen in 1889-1890.¹

The most obvious feature that distinguishes Tylor's work from that of his English contemporaries and successors is the universality of his ethnological interests. Others, like Lang and Frazer, were predominantly occupied with sociological and religious problems; Tylor's vision embraced, to cite his own definition of culture, "that complex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits

¹ The biographical data are taken from Lang's sketch in the anniversary volume cited above and from Professor Alfred C. Haddon's obituary notice in *Nature*, January 11, 1917, p. 373.

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acquired by man as a member of society." He was equally attracted by the description of a Malagasy bellows and by an account of the South American couvade, by the process of stone-boiling and by solar mythology.

In Tylor's attitude towards the immense mass of concrete fact with which his versatility brought him into contact a distinctive psychological trait is manifest—his intuitive sense of fitness. We must recall the character of the data available when he commenced his life work—the hodge-podge of imperfect observation and provincial bias with which he was obliged to deal in order to get at the mere facts. To be sure, there was excellent material by men like Cranz, Sahagun, or Callaway. But even the most reputable of the older writers were prone to state as fact what was either crude misinformation at second-hand or crude misinterpretation due to the colored spectacles of European civilization. What shall we say when we find Burton declaring that the Arapaho possessed so scanty a vocabulary that they could hardly converse with one another in the dark when gestures were invisible, or Baker denying any form of religion to the aborigines of the Upper Nile region? In the evaluation of such utterances Tylor showed an almost unerring instinct, all the more commendable since many of the wild statements of this type would have fitted admirably into that general evolutionary scheme of the universe which he himself was helping to develop.

This critical judgment was apparent in the discussion of problems as well as in the weighing of travelers' accounts, but here the result was not so uniformly satisfactory. Indeed, the question obtrudes itself, whether Tylor's famous caution was not sometimes conformity to a scientific ethical ideal of fairness in discussion rather than a trait inherent in his mental make-up. He certainly carried the judicial weighing of pros and cons to an exceptional degree. On rereading the *Researches into the Early History of Mankind*, I can understand Wallace's irritation at its indecisiveness and Lubbock's misunderstanding of the argument as to the single origin or independent development of the couvade. But whatever formal hedging there may be in the marshaling of arguments, the conclusion sometimes appears as a thunderbolt out of a blue sky, as when historical connection is used to interpret

the existence in remote areas of the cure by extracting pathogenic agents from the patient's body.

This illustration, however, brings up a topic which shows Tylor to the greatest possible advantage in historical perspective. Though certainly a strong believer in the independent evolution of cultural phenomena in distinct areas of the globe, he was very much alive to the influence of diffusion. In the Introduction to the English translation of Ratzel's *History of Mankind* he contrasts "the small part of art and custom which any people may have invented or adapted for themselves" with "the large part which has been acquired by adopting from foreigners whatever was seen to suit their own circumstances." Indeed, in many concrete instances he goes much further than at all events modern American ethnologists are inclined to follow. The case of cure by suction has already been cited, while another chapter of the same book pre-figures in principle the recent hypothesis of a cultural connection between aboriginal America and the Old World. Whatever we may think of particular interpretations offered by Tylor, the traditional American conception of him as merely an evolutionist of the classical school is ridiculously false. His suggestive and indeed conclusive discussion of the Malagasy iron technique alone suffices to show what a valuable tool he sometimes made of the principle of historical connection.

Nevertheless, it remains true that Tylor's name will always be most prominently connected with the doctrine of evolution. In this context it is very cheap to assume an unhistorically critical attitude. We must recollect that just as he had to sift the chaotic mass of ethnographic observations in order to extract the actual facts so in the interpretation of culture history he had to contend with a powerful, theologically inspired theory of degeneration against which the principle of progressive evolution had to be established and defended. To have accomplished this task so effectively is in itself no mean achievement to Tylor's credit. But Tylor further enriched the doctrine of cultural evolution by the development of a definite and elaborate scheme for the subject of religion. To enter into a discussion of this theory of animism is out of the question within the limits of this notice. Suffice it to say that as presented in *Primitive Culture* it remains, in spite of

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all criticism, the most impressive theory of primitive religion yet advanced.

To philosophical ethnology Tylor contributed the concept of survivals and the intimately associated method of "adhesions" outlined in his ever memorable paper "On a Method of Investigating the Development of Institutions, Applied to Laws of Marriage and Descent," which was presented to the Anthropological Institute in November, 1888, and published in Vol. XVIII (1889) of its *Journal*. It must be reckoned a distinct loss to science that the complete data on which this lecture was based were never published. The fundamental idea is the application of statistical methods to the data of ethnography. If two or more cultural traits are repeatedly found in association, are we dealing with a chance combination or is there an organic correlation? Tylor compares the number of times such combinations might be expected to occur on the doctrine of probabilities if each feature were independent of the others with the number of occurrences empirically found, and where the latter is clearly in excess he infers a causal connection. In this manner, e.g., he establishes a functional relationship between the exogamous dual organization and the classificatory systems of kinship terminology, between the parent-in-law taboo and matrilocal residence, and between the couvade and a mixed maternal-paternal organization.

The very idea of introducing into a branch of knowledge that is so often the happy hunting-ground of the curiosity-seeking dilettante something of the rigor of the exact sciences is one of well-nigh unparalleled magnificence. Nothing that Tylor ever did serves so decisively to lift him above the throng of his fellow-workers. Without that paper he might have ranked as a sort of super-Lang or super-Frazer—more universal in his grasp than either, more serious and erudite than the one, far more trustworthy in his judgment than the other. But the paper on Method raises him at once into an entirely different category of intellectual being.

In the appraisal of this contribution several points should be considered separately. In the first place, quite apart from the main argument, Tylor here first conceptualized certain phenomena which have since loomed more or less prominently in ethnographic

literature, viz., teknonymy and cross-cousin marriage. Secondly, he was fully aware of the fact that it is one thing to establish the mere fact that two features are causally related and quite another to determine the reason for the association. The former is by far the more important methodologically and whatever criticism may be advanced against Tylor's specific conception of the nature of the correlation does not affect the core of the method. This likewise remains valid even if we reject the evolutionary interpretation which Tylor gave to certain of his observed correlations. Finding no instances of the *couvade* among matrilineal tribes, twenty cases among peoples with a mixed system, and eight in patrilineal communities, Tylor not only inferred that the institution had originated in the mixed system and dwindled away with paternal descent but also that this established the priority of matrilineal descent. Obviously, this conclusion does not follow from the empirical facts of correlation but already involves the acceptance of a unilinear scheme of evolution.

The essential objection to Tylor's paper, as pointed out in the oral discussion by Galton and Flower, rests on his neglect of diffusion. If the same combination recurs a hundred times among tribes that have had no historical connection, we have indeed established a rule of organic correlation; but if the combination has been disseminated from a single point of origin there is no means of proving that we are dealing with more than a mere chance association. We in America who accept diffusion to a considerable extent but at the same time admit independent development are confronted with the fact that exactly the same usages are found in remote regions of the globe between which any connection remains unproved. On the other hand, these similarities do seem to go hand in hand with certain other similarities, with which therefore they seem to be functionally related. This means that where one of the traits occurs we can legitimately infer its one-time association with the correlated trait. We must insist against Tylor that the particular tribe in question may have borrowed the feature isolated from its old context; but to assert that such a correlation as that between the avunculate and a matrilineal organization is due to sheer chance is ridiculous, more so than the wildest Graebnerism, which at least does not blink at

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the observed fact of complete cultural identity. The best evidence for such an organic correlation seems to me to have been advanced in the field of kinship nomenclature, where Tylor himself established the relation of the classificatory system with exogamy. But the method is applicable to an indefinite number of similar problems, and ethnologists will do well to turn to Tylor's extraordinarily stimulating and fruitful mode of investigation.

Over and above his specific contributions, Tylor had a clear vision of the place of ethnology in modern civilization. The facts of primitive life were to him not mere specimens for a museum of psychological oddities nor was he altogether satisfied with using them as bricks for a theory of cultural development. Beyond its academic aspects he maintained that "such research has its practical side, as a source of power destined to influence the course of modern ideas and actions." The sight of mankind painfully groping through the ages from the crude fist hatchet to modern technology must inspire active endeavor to add to the heritage of the past. But ethnology also reveals in modern law, ethics, and theology innumerable survivals from primitive savagery, which it marks out for destruction, being in Tylor's own words "essentially a reformer's science."

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ON THE FIFTIETH ANNIVERSARY OF LEWIS H. MORGAN'S death Russian anthropologists held a meeting in his honor; and The Academy of Sciences of the U.S.S.R. is publishing *Ancient Society* in its "Classics of Scientific Thought." Morgan's work is officially proclaimed as "of paramount importance for the materialistic analysis of primitive communism," while his critics are taunted with their bourgeois prejudices. But naturally his results "could not fail to fill with anxiety the hearts of those who connected their fate with the preservation of such relations and conceptions as were congenial to them."¹

Attempts to rehabilitate Morgan, however, are not restricted to Soviet philosophers. A Hindu writer dubs him "the Tylor of American anthropology," and a *greater* Tylor at that. Morgan, who found all primitive religions "grotesque," is quaintly credited with a deeper insight into religious phenomena; and still more

Essays in Anthropology Presented to A. L. Kroeber (Berkeley: University of California Press, 1936), pp. 169-181.

¹ N. Matorin, "Soviet Ethnography," in *Ethnography, Folklore and Archaeology in the USSR*, IV (1933), 6; E. Kagarov, "The Ethnography of Foreign Countries in Soviet Science," *ibid.*, p. 89.

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oddly, Tylor's method of "putting together disjointed scraps of things pertaining to man in one basket" is contrasted with Morgan's quest of "the grand harmony" in otherwise meaningless facts.²

Morgan evidently remains more than a merely historic figure. It is worth while, then, to reassess his contribution and his influence in historical perspective. The task is difficult, but it is lightened by Dr. Stern's labors,³ which make it appreciably easier to relive the intellectual situation of Morgan's epoch.

Morgan is profitably considered under three heads: as a gatherer of facts, as a philosopher of culture history, and as a contributor to the field of social organization.

As an ethnographer, Morgan takes high rank. One naturally thinks first of his *League of the Ho-de-no-sau-nee or Iroquois* (1851), but the results of his very brief visits to more remote tribes are likewise most creditable. He discovered the matrilineal exogamous clan organization of the Crow, an observation once doubted but wholly confirmed by later research; and he registered sororal polygyny as a Crow usage. Exactly as I did some decades later, he noted that men and women chopped off a finger joint in mourning or as a religious sacrifice. What is more, his description of the Crow kinship system is vastly superior to my original attempt in this direction, for he recognized that cross-cousins were put into different generations from the speaker's. As I subsequently wrote: "My error seems the less pardonable because the essential facts had already been grasped by Morgan."⁴

Morgan's honesty as a field worker is no less conspicuous than his acuity. According to his theoretical scheme, the Dakota ought to have been organized into clans ("gentes" in his terminology); yet, we learn in the early 'sixties he himself "could find no satis-

² Panchanan Mitra, *A History of American Anthropology* (Calcutta, 1933), pp. 109-120. The phrase from Morgan is in his *Ancient Society* (New York, 1877), Part I, chap. 1.

³ Bernhard J. Stern, *Lewis H. Morgan, Social Evolutionist* (Chicago, 1931); *idem*, "Selections from the Letters of Lorimer Fison and A. W. Howitt to Lewis Henry Morgan," *American Anthropologist*, XXXII (1930), 257-279, 419-453.

⁴ Morgan, *Ancient Society*, Part II, chap. 6; J. R. Swanton, "The Social Organization of American Tribes," *American Anthropologist*, VII (1905), 663-673; R. H. Lowie, *Notes on the Social Organization and Customs of the Mandan, Hidatsa, and Crow Indians*, AMNH, Anthropological Papers, XXI (1917), 21-26 f., 56.

factory traces of gentes among them." Again, he registers similar failure among the Athabaskans of Hudson's Bay territory, and the inability of another investigator to discover them for Morgan in the Slave Lake area.

It must be admitted that Morgan was not uniformly interested in the whole of culture. Like most recent investigators, he devoted himself intensively to particular aspects of native life and skimmed over others. As Stern remarks, his *League* tells a great deal about Iroquois social organization, but, *pace* Mr. Mitra, little about religion, and economic life is treated inadequately. However, Morgan did not neglect technology, and altogether his account remains an outstanding achievement, both for its fullness and the sympathy evinced toward his subjects.

As a culture historian, Morgan was handicapped by lack of essential facts. Not, however, in the light of recent discoveries but as a contemporaneous verdict we must uphold Lubbock's stricture in 1878 that "Morgan's knowledge is anything but exhaustive." He was indeed incomparably ahead of his times along special lines, yet even there his neglect of accessible sources distorted the picture he gave of ancient society. Though he had himself noted the Iroquois fraternities, he never considered the relationship of clubs, military organizations, or religious corporations to the social structure of American natives, let alone of Melanesians or Africans. Yet Maximilian's account of the Mandan warrior societies was available in English and German; and their police activities certainly had some bearing on that "Idea of Government" to which Morgan dedicated fifteen chapters of *Ancient Society*.

Equally astonishing is Morgan's neglect of aristocracy and monarchy among ruder peoples. He laid it down as an axiom that monarchy was incompatible with clans, that it appeared only in "civilization," that is, in the period of literacy. Aristocracy, he argued, did not develop before the "Later Period of Barbarism," that is, not before the Iron Age.⁵ This dogmatism happened to yield a valuable by-product—the critical scrutiny of the Spanish chronicles with their extravagance about a feudal Aztec empire. But the general propositions were wide of the

⁵ Morgan, *op. cit.*, Part II, chap. 5; Part IV, chap. 2.

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mark. Had Morgan never heard of the African courts described by early travelers? Was he ignorant of Chaka's spectacular conquests in the early nineteenth century? Had he ever peeped into Captain Cook's *Travels*? But it was not even necessary to go so far afield. Caste distinctions and slavery were well-established phenomena among the natives of British Columbia. Since Morgan's honesty is beyond reproach, failure to note such facts can be imputed only to sheer ignorance. Evidently indefatigable in ferreting out what was relevant to clan systems and relationship terms, sparing no pains to acquire the kinship terminology among Hawaiians and Rotumans, Maori and Samoans, he seems to have had no inkling of the class distinctions that prevailed in Polynesia. How did he interpret Tylor's statement that in eastern Asia and Polynesia the names of kings and chiefs were held sacred? ⁶ Probably few men were further than Morgan from a "functional" view of different cultures as so many living wholes.

From this deficiency springs one of Morgan's worst errors of classification, the inclusion of the Australians and "the greater part of the Polynesians" in the "Middle Status of Savagery." To be sure, the Australians "rank below the Polynesians," but because they lack bow and arrow, Maori and Kurnai are both placed below the Northern Athabaskans.

Psychologically, a flair for individual cultures and historical-mindedness naturally go together. Morgan lacked both; his conclusions had a bearing on time sequences, but his chronology did not rest on archaeological stratification or written records, but on an abstract scheme. The peoples of the world are classified "according to the degree of their relative progress" into three periods, Savagery, Barbarism, and Civilization, the last being characterized by literacy. The two earlier periods are each subdivided into a Lower, Middle, and Upper Status. The Lower Status of Savagery was avowedly hypothetical, ending with the use of fire and the acquisition of a fish subsistence. The Middle Status terminated with the invention of bow and arrow; Australians and most Polynesians thus fall into this rubric. The Upper Status of Savagery ended with the manufacture of pottery, thus embrac-

⁶ E. B. Tylor, *Researches into the Early History of Mankind and the Development of Civilisation* (London, 1865), p. 142.

ing the Indians of northern North America. Barbarism begins with pottery, and its Upper Status coincides with a preliterate Iron Age.

In the abstract, Morgan admitted his tests only with qualification. But in dealing with concrete material he acted precisely as if they were absolute. Specifically, he has sufficient confidence in them to deduce unknowable history from the scheme. This appears precisely where up to a point Morgan did avail himself of written documents. So far as a general ethnologist can judge, he studied the sources on Greek and Roman society with painstaking zeal. But while setting forth the records for the historical period, he goes far beyond ascertainable facts. According to him exogamy was characteristic of the Greek *genos* and the Roman *gens*, "a novel doctrine," as Tylor remarked in 1878, "which his evidence fails to establish."⁷ What is more, Morgan admits "the absence of direct proof of ancient descent in the female line in the Grecian and Latin *gentes*," but follows Bachofen in assuming that it once existed. "It is impossible to conceive of the *gens* as appearing, for the first time, in any other than its archaic [i.e., matrilineal] form; consequently the Grecian *gens* must have been originally in this form." That is to say, granted a universal *law* of progression, that law necessarily applies to Greek and Roman, as well as to other cultures. If, however, laws give us the essence of history, why trouble at all about a piecemeal study of the detailed course of events in particular societies? Here lies the inevitable conflict of the "unilinear evolutionist" and the culture historian. To the evolutionist it is obvious that progress has been "substantially the same in kind in tribes and nations inhabiting different and even disconnected continents, while in the same status." Hence the conclusion that the archaic structure of Greek and Roman society "must even now be sought in the corresponding institutions of the American aborigines." The historian's approach is radically different. Dealing with observed sequences, he is quite prepared to find diverse trends among diverse peoples. When Laufer discusses the development of art in India, he explicitly points out that it is wholly different from that of the Chinese;

⁷ Quoted in Stern, *op. cit.*, p. 141.

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while Chinese painting evolved from calligraphy, the lack of ornamental penmanship precluded parallelism in India, where the science of physiognomics played a corresponding part.⁸

Of course, all the eminent evolutionists knew that deviations from a norm of progress were inevitable; and Tylor expressly stated that most of human culture had "grown into shape out of such a complication of events, that the laborious piecing together of their previous history is the only safe way of studying them." Far from always asserting independent development, he anticipates many diffusionist conclusions that are now generally accepted. He derived the Malagasy bellows, the Andamanese outrigger canoe, from Malaysia; he envisages a single origin for the bow and arrow; he supports the view that North American pottery "spread from a single source."⁹ Tylor did not, as has been erroneously alleged, abandon this view in later years, but maintained it in discussing the patolli game in the anniversary essays in honor of Bastian. Indeed, that paper discusses the methodology of diffusionism; and the only reasonable criticism of Tylor is that he did not uniformly balance the contradictory principles of explanation. Notably, his famous statistical discussion fails to consider the effect of transmission on the course of organic development.

To turn back to Morgan, he was not wholly content with the patter of the day. At least sporadically he asks himself what may be meant by similar causes: "The phrase 'similar conditions of society,' which has become technical, is at least extremely vague. It is by no means easy to conceive of two peoples in disconnected areas, living in conditions precisely similar."¹⁰ He was, indeed, fond of mentioning "the unequal endowments of the two hemispheres" as explaining the cultural differences in the same period, namely, the possession of domesticable animals that could furnish meat and milk to Old World peoples. Here, once more, Morgan was hampered by ignorance. It was no

⁸ Berthold Laufer, *Das Citralakshana, nach dem tibetischen Tanjur herausgegeben und übersetzt* (Dokumente der indischen Kunst, Heft I, Malerei; Leipzig, 1913), p. 32; Morgan, *op. cit.*, Part I, chap. 1.

⁹ Tylor, *op. cit.*, pp. 4, 167, 366.

¹⁰ Morgan, *Systems of Consanguinity and Affinity* (Washington, 1871), pp. 472 f.; *idem*, *Ancient Society*, Part I, chaps. 1 and 2.

secret in 1877 that the same domesticated animals were quite differently used by the Chinese and the Egyptians or the pastoral nomads; in other words, that "equal endowments" did not automatically evoke similar cultural responses. In any case, whatever transitory qualms may have arisen in Morgan's mind about the improbability of frequently similar conditions, they did not impress his philosophy of progress. The very sentence that points out the difference of the Old and the New World terminates with the clause: "but the condition of society in the corresponding status must have been, in the main, substantially similar."

As to diffusion, Morgan's position was strange. In his system the Upper Status of Barbarism begins with the Iron Age. Yet he puts the ancient Britons, "although familiar with the use of iron," into the *Middle* Status. "The vicinity of more advanced continental tribes had advanced the arts of life among them far beyond the state of development of their domestic institutions." As Stern remarks, this exposes the weakness of the entire scheme. It rests avowedly on "such . . . inventions or discoveries as will afford sufficient tests of progress," that is, on the arts of life. Now we suddenly learn that arts of life can spread by borrowing without affecting cultural status. But the very same principle applied to ancient Britons can be logically applied to an indefinite number of other cases. The vicinity of bow-using peoples might explain the archery of the Northern Athabaskans, who on other grounds might well be degraded to the Middle Status of Savagery, the level of Australians *and* Polynesians (*sic*); and so forth.

In short, Morgan never considers what remains of a scheme of development if warped by incessant borrowing. Since every human group has been exposed to unique outside influences from time immemorial, how can we maintain that "the experience of mankind has run in nearly uniform channels?" Some manner of evolutionary parallelism might still be rescued from the débris, but surely its limitations should be recognized.

It is no reproach to Morgan that he failed where Tylor fell short of complete clarity, where we of today are still floundering. But the special ways in which Morgan invoked diffusion are objects of legitimate wonder. He regarded the clan ("gen-

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tile") organization as nearly universal; and from an archevolutionist convinced of a "logical progress" in human institutions we should expect that this would serve as a sample of what "the natural logic of the human mind" would everywhere produce in similar circumstances. Yet Morgan takes precisely the opposite position: the clan is treated as so "abstruse" a conception that a single origin is postulated. The biological advantages of exogamy were such, it seems, that this type of organization "would propagate itself over immense areas."¹¹ I am aware that the multiple origin of clan systems remains a moot problem; but nowadays the theory of a single historical source is linked with a general diffusionist bias, not with a belief in evolutionary laws.

Odd as such rank diffusionism appears in its evolutionist setting, it is almost mild compared with Morgan's intransigence in dealing with his favorite kinship data. Mere diffusion is not deemed sufficient here; borrowing, Morgan argues, would have involved the taking over of the very terms themselves. Hence, racial affinity alone can explain resemblances in the classification of relatives. Because the Hawaiian and Zulu nomenclatures share certain features, the Polynesians and the Kaffir must have sprung from the same stock; because the Tamil of India and the Seneca in New York state have similar systems of relationship, the affinity assumed in the name American *Indian* stands ultimately justified.¹²

This extraordinary intrusion of a biological factor to explain linguistic and sociological data cannot be treated as in any way due to contemporary currents of thought. Lubbock, for instance, at once demolished Morgan's conclusions. The Seneca resemble not merely the Tamil but also the Fijians and Australians in regard to relationship systems; are they, then, specifically related to all these races? Still more decisively the critic points out that on this basis the Two-Mountain Iroquois would have to be considered racially closer to the Polynesians than to the other Iroquois tribes.¹³

¹¹ Morgan, *Ancient Society*, Part II, chap. 15.

¹² *Idem*, *Systems*, pp. 500-508.

¹³ Lord Avebury, *The Origin of Civilisation and the Primitive Condition of Man* (London and New York, 1911), p. 179.

This case is especially illuminating. Morgan was here dealing with material he mastered incomparably better than any of his contemporaries. Yet the historical conclusions he arrives at are manifestly absurd on the basis of the data he had himself presented. No better proof can be required of his deficiency in historical tact. Again, the contrast with Tylor is startling. The numerous instances of transmission Tylor adduces, especially in the *Researches*, are not all valid; but none of them is plainly absurd, many remain suggestive, if not convincing.

Turning now to Morgan's specifically sociological work, we cannot divorce his interpretations from either his raw material or his philosophy of progress. As noted, his picture of society was inevitably imperfect from neglect of associational phenomena, a gap not filled until Heinrich Schurtz's *Altersklassen und Männerbünde* (1902). It was further marred by the biographical accident that Morgan's contacts with aborigines began where they did—among the democratic clan-organized Iroquois. I have often wondered what his scheme might have been like if chance had first thrown him among the clanless Paiute, the wealth-craving Yurok, the pedigree-mad Polynesians, or the monarchical Baganda. Proceeding from the Seneca and encountering for hundreds of miles nothing but broadly comparable social structures, Morgan prematurely generalized what primitive society was like, even though on an apparently wide inductive basis. And when he had once formulated the generalization, he could dismiss contradictory evidence about the Columbia River tribes with the cheap auxiliary hypothesis that their clan organization had fallen into decay. I say "cheap" advisedly, because the proper procedure—whether definitive or not—on Morgan's own principles would have been to examine the kinship systems, which he believed always embodied evidence of preëxisting social conditions.

On the positive side we note, first, that Morgan personally secured much of the North American material, either directly or by correspondence; and that his outline of North American social organization remained for decades the only comprehensive summary. Indeed, imperfect as it is, no adequate substitute in the light of present knowledge has yet been provided. Secondly, Morgan was not indeed the first to conceive the distinction

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between territorial and kinship organization, but he grasped Maine's important conceptualization (1861) and made it underlie his own treatment of social development. In my judgment, Maine and Morgan err in completely denying local ties among primitive groups, but doubtless these bonds are comparatively weak; and whatever the ultimate verdict may be, Morgan deserves credit for recognizing the enormous importance of this luminous distinction.

A genuine contribution was the clearing up of the concept "exogamy." Contrary to his wishes, Morgan did not, indeed, succeed in banishing the term from scientific nomenclature, but his critique of J. F. McLennan in *Ancient Society* made it possible to use it intelligently with reference to observed facts. McLennan confused the issue by speaking of tribes as endogamous or exogamous, respectively. Morgan explained that the clan, a subdivision of the tribe, was the exogamous unit; and that marriage enforced outside this unit normally took place *within* the tribe, so that "both practices exist side by side."

Another valid stricture advanced against McLennan related to his representation of polyandry as a general phenomenon at a certain stage, while Morgan rightly regarded it as exceptional—a view shared with Lubbock.¹⁴

Various views popularly associated with Morgan need not be discussed in detail, because they were not in any sense peculiarly his. Thus, the priority of maternal over paternal descent had already been postulated by J. J. Bachofen and become part of the scientific credo of the period. Lubbock, Tylor, and Lang were only a few of the scholars who supported this view. Again, the notion that individual marriage was inconceivable among rude savages and must have been achieved by gradual progressive stages beginning with sexual communism was merely contemporary doctrine.¹⁵

For the same reason I can see nothing remarkable in Morgan's ideas on property. According to Professor Matorin, to be sure, Morgan's investigations "have proved the communistic character of the primitive community and have filled with mortal fear the hearts of all obscurants, all those who are guarding the

¹⁴ *Ibid.*, p. 151.

¹⁵ *Ibid.*, p. 103.

foundations and pillars of traditional morality.”¹⁶ If, however, the Russian scholar had looked into Lubbock, he would have found much the same position as in Morgan—a firm conviction that individual land ownership was always preceded by a period in which the land was common.¹⁷ Morgan, of course, attempted to fit specific ideas on this topic into his general scheme. Thus, we learn that savages owned nothing but rude chattels, hence had not yet developed a passion for their possession; that at first children inherited only from their mother; that slavery—a well-established institution on the Canadian West coast—only sprang into being in the Upper Status of Barbarism, that is, in the Iron Age. At best commonplace, the section of *Ancient Society* devoted to the “Growth of the Idea of Property” is at its worst vitiated by the complete neglect of aboriginal slavery, aristocracy, and monarchy.

On the other hand, Morgan, though not ahead of his time in this respect, cannot fairly be criticized for ignoring the innumerable instances of incorporeal property that at once nullify the theory of primitive communism, since the relevant facts failed to arouse interest until a much later date. However for his servile followers of today the excuse will not hold. It has, indeed, been asserted that primitive “copyrights” are economically insignificant, having merely sentimental value for their holders. But this allegation is demonstrably false. A Plains Indian who purchases a sacred bundle can readily transmute it into the most material of economic goods; he is making the safest of investments for the future. Similarly, the Eskimo, communistic as they are in the distribution of food, are rabid individualists when it comes to magic formulae. “Those who possess the words will not part with them, or if they do, it is at a price which would soon ruin an expedition.” Rasmussen found an old woman who had taught a fellow tribesman her spell; in return he provided her “with food and clothing for the rest of her life.”¹⁸ In modern parlance, she had bought herself an annuity.

¹⁶ Maturin, “Soviet Ethnography,” in *Ethnography, Folklore and Archaeology in the USSR*, IV (1933), 6.

¹⁷ Avebury, *op. cit.*, p. 478.

¹⁸ Knud Rasmussen, *Intellectual Culture of the Iglulik Eskimos* (Copenhagen, 1929), p. 165.

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If Morgan deserves no more credit than a dozen other writers for being stimulated by biological evolutionism, he achieved unique distinction by his treatment of kinship; his fame rests securely and primarily on the Systems of Consanguinity and Affinity. In order to visualize the greatness of his achievement, we must first picture to ourselves the status of relevant problems before Morgan. Missionaries and others had, of course, noted that primitive peoples classify relatives according to non-European norms. But prior to Morgan no one had seen a problem in such exotic usage; no one had systematically garnered the several nomenclatures, compared them with one another, attempted a typology or interpretation. Morgan, evincing that "eye for essential fact" which even Lubbock was willing to concede, devoted twenty years to assembling the pertinent facts, partly by personal field studies, partly through the services of numerous correspondents. The result is a mass of raw material incomparably fuller than anything yet brought together by any of his successors. The work represented the terminologies of 139 distinct tribes or peoples and presented them, on the whole, in the greatest detail. Anyone who has worked out a single system in the field knows what is implied in this statement. Morgan was inevitably limited by the state of ethnographic knowledge. Africa was still the Dark Continent, South American information was scant or buried in inaccessible sources, from which it is only now being disengaged by men like Kirchhoff. Having regard to the contemporary situation, Morgan must be credited with sparing no pains to achieve a world survey, in which he included Indo-European and Semitic as well as native peoples.

The case is illuminating as to Morgan's psychology. Nothing is less apt than Mr. Mitra's rhetorical flourish that "like a colossus he strode in every field of anthropology." The truth is that in most fields he did not stride at all. His contemporaries—say, Lubbock—dealt with art, language, and religion; on none of these topics can we find any enlightening general ideas in Morgan's writings. His interests were narrowly focused, but when he dug, he dug deep. And the result is clear. Lubbock's versatile mind, outside of prehistory, has left little mark on his

successors; while in the one subject to which he devoted himself—social organization and especially kinship terms—Morgan remains a towering figure. His work has been revised and amplified, but it cannot be ignored.

Naturally, the mere assemblage of raw data would never confer the title of greatness; nor, may we add, would Morgan ever have collected them on so vast a scale unless goaded by the search of general principles. It is here, in the appraisal of Morgan's interpretations, that discrimination is imperative. For, as we have already seen, the assumption of racial affinities because of terminological resemblances was not merely wrong, but absurd in the light of Morgan's own data.

Intermediate, however, between ultimate explanation and the accumulation of mere data comes the process of classification and conceptualization. The qualities Morgan displayed here were those characteristic of him in general—not subtlety, but painstaking industry and rugged intelligence. In contrast to most of his successors he saturated himself with the chaotic mass of fact, and thus, quite apart from his main purposes, was able to foreshadow much of the subsequent typology. He noted the aberrant features of the Crow-Hidatsa nomenclature and its parallels elsewhere; he personally discovered the "Omaha" type of system among the Kansas and indicated its distribution; imperfectly informed as he was about our Western tribes, he detected such anomalies (from the Iroquois angle) as the distinction of paternal and maternal grandparents and the use of reciprocal terms.¹⁹ Incidentally Morgan thus provided a solid basis for distributional studies and discoveries of historical connection, even though of an order less pretentious than that he consciously envisaged.

In the conceptualization of types of relationship terminology Morgan was not wholly successful. He was hampered by defective knowledge of those very tribes whose systems had a crucial bearing on the problem of classification. The data from Eastern and Central North American tribes, eked out by Oceanian and Australian reports, inevitably encouraged the inference that all aboriginal tribes grouped relatives into large classes. Hence,

¹⁹ Morgan, *Systems*, pp. 179, 188, 211, 245, 247, 252 f., 262.

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Morgan's main category of Classificatory Systems, to which he opposed that of Descriptive Systems as supposedly characteristic of the Indo-European family. Rivers pointed out that "descriptive" was not an apt term since most English terms, for instance, are denotative rather than descriptive. To be sure, Morgan's contention was rather that the *original* "Aryan" system was descriptive; that, like Erse, it had denoted a few primary relationships by specific nouns and described all others, that is, "son of brother," "son of son of brother." But obviously even on that assumption Rivers was right in objecting that modern Indo-European languages for the most part failed to fall into Morgan's rubric. From the opposite side Kroeber has urged that English and related tongues are not devoid of classificatory terms, such as "cousin." To me, however, there seems to be a still more vital objection. "Descriptive" designates a *technique* for defining relationship, "classificatory" a mode of grouping. The two concepts are thus not mutually complementary but relate to different subjects of discourse. Hence, a descriptive phrase, say, father's brother's son, might quite conceivably be applied to an indefinite number of individuals.

At present we are, indeed, still far from a satisfactory grouping of nomenclatures, but certain conclusions seem definitive. A nomenclature of relationship is not usually one system, but the result of several crisscross currents: as Kroeber phrases it, there are a number of diverse categories.²⁰ Morgan fully realized the significance of *one* category, that of grouping collateral with lineal kindred, but he rather naïvely assumed that features which merely chanced to appear with this fundamental trait were organically bound with it. Thus, the separation of junior from senior siblings constantly figures as a criterion of "classificatory" systems. Yet obviously the aborigines in question are not thereby making classes larger than ours, but on the contrary are introducing distinctions foreign to us. If the term has any significance at all in such a context, it is we who are classificatory; and, however that be, the segregation of senior from junior siblings is not possibly related—except by historic ac-

²⁰ A. L. Kroeber, "Classificatory Systems of Relationship," *RAI, Journal*, XXXIX (1909), 81.

cident—to the inclusion of parallel cousins with siblings or of paternal uncles with the father.

Morgan's strength and weakness appear clearly in his treatment of the Eskimo "system." With exemplary candor he points out that eight of the ten features indicative of the North American Indian terminologies are lacking. Nevertheless, he defines it as classificatory. "It is . . . a classificatory as distinguished from a descriptive system. But in the greater and most important fundamental characteristic of this system it is wanting. The Eskimo form not only fails in the necessary requisites for the admission of this people, upon the basis of their system of relationship, into the Ganowanian family, but furnishes positive elements to justify their exclusion." There may possibly be, he argues, an ultimate, there is certainly not an immediate, relation of the systems; indeed, the Eskimo nomenclature approaches the Aryan and Uralian more closely than it does that of the American Indians.²¹

Comparing this statement with Frazer's glib reference to the Eskimo terminology as "classificatory," we at once recognize Morgan's immeasurable superiority. There is all the difference in the world between a bowing acquaintance and a serious grappling with refractory kinship data. Morgan has not achieved conceptual clarity, but he knows that here is something different from the Seneca norm, something so different that it verges on the exact opposite of a classificatory system, even if he cannot quite muster up courage to call it "descriptive."

Apart from his blind spot as to racial affinities, Morgan by concentration on his schedules was preserved from errors to which more casual investigators have fallen prey. His rebuttal of McLennan, for example, refutes the quaint notion that kinship through females implies ignoring of patrilineal kin, a conclusion apparently not yet universally accepted.

The distinction of Morgan, then, is not simply that he heaped up vast stores of information on a subject of theoretical import, but that he immersed himself in this welter of fact, came to grips with it, *thought* about it. The specific quality of his thinking shows to advantage in the discussion of the clan organiza-

²¹ Morgan, *Systems*, pp. 277, 470, 510.

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tion as a possible cause of the Seneca type of classificatory system. He examines the implications of such an organization and conclusively demonstrates that, while it would account for the identification of sisters' children with siblings under maternal descent, the similar identification of brothers' children would remain unexplained.²² So, of course, it does except in the specialized case of exogamous moieties.

Of Morgan's scheme for the evolution of the family, little need be said, because, as already suggested, it contained little that distinguished him from the other evolutionists. Promiscuity naturally came first, but avowedly not as anything but a theoretical deduction; the monogamous family came last; and there were intermediate stages bridging the gap. Morgan's originality lay in bringing this scheme of stages into correlation with forms of kinship terminology. That he committed a number of grave errors in this connection is pretty generally admitted. In harmony with the spirit of the times he took it for granted that what was simpler must be older, hence inferred that the Polynesian nomenclatures are more archaic than their American equivalents. He made a really fatal error in supposing that when a Polynesian addressed his father and, say, his mother's brother by a single term this implied conceiving the uncle as a possible procreator. Objectively formulated, the fact simply is that one common term applies to the begetter and other male relatives, that the Polynesians lack a term denoting paternity in our sense. Hence, Morgan's conclusion that the maternal uncle once mated with his sister (the speaker's mother) is fallacious: that custom would indeed logically produce the observed classification, but it is not the only possible usage that can lead to this result. Members of the same sex and generation may simply be grouped together under a blanket status term. Morgan's mistake, then, lies in misinterpreting the import of the aboriginal facts by reading a modern meaning into the translated kinship terms and in ignoring alternative determinants.

When, however, we discount a pioneer's pitfalls and the warping due to contemporary bias, a magnificent and valid conception remains. Lists of relationship terms are lexical elements devoid

²² *Ibid.*, p. 476.

of interest except to a linguist unless they are brought into contact with the elements of reality to which they refer. What is more, the linguistic approach culminates not in an explanation of the phenomena, but in a negation of the possibility of explanation. Kinship nomenclatures are certainly amenable to the changes which affect words, and since these changes are capricious we can never hope to reduce all features of relationship terminology to social antecedents. However, insofar as they are explicable at all they must be explained on sociological lines. The Southern Siouans and the Miwok of central California belong to diverse linguistic families, are separated by a distance of well over a thousand miles, are not known ever to have lived in close proximity. Their nomenclatures share the "Omaha" features of classing a mother's brother's son with the maternal uncle—a feature lacking in all the intervening tribes. Do the Southern Siouans resemble the Miwok more than their fellow Siouans of Montana and Dakota because of a miracle, or is it because both Southern Siouans and Miwok demonstrably share forms of marriage and linked social customs not found among the Crow and Teton? The choice lies between a sociological interpretation and the abandonment of interpretation.

We thus find Morgan's major postulate vindicated that kinship terminologies in some measure correspond to social facts, among which matrimonial rules are prominent. However, Morgan also asserted that social custom advanced while its lexical equivalents remained stationary; the use of a kinship term thus may point to the prior existence of an obsolete usage. Some who accept the correlation of social custom with nomenclature balk at this application of the principle of survivals.

- ✓ Trained to view "survival" arguments with suspicion, I have become convinced that the avowed skepticism on this point harbors as much cant as the evolutionary zeal of our predecessors. Indubitably cultural changes proceed with uneven velocity, hence certain elements lag while others spurt ahead; further, linguistic phenomena are markedly conservative. These accepted facts warrant the assumption that a terminological feature in harmony with a certain custom may survive that custom. The only question is whether the social factor is the only possible

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determinant, whether the really vital factor is not rather one of its correlates, whether the same result may not be effected by a different cause. But when due allowance is made for this, Morgan's principle of survivals remains a valuable procedure.

In accepting the independent repetition of terminological features where social concomitants are repeated, we avowedly admit "evolution." However, it is a strictly empirical parallelism, which does not pretend to sketch culture history as a whole, but merely to account for specific resemblances. It is, indeed, very difficult to deal with cultural phenomena and fail to recognize certain organic bonds between phenomena. As Father Schmidt has recently explained, aprioristic evolutionism must be eschewed, but it is quite proper to make a "quite logical deduction from the very nature of things and men, to arrange them [cultural measures] in a certain series of phases of development."²³ Morgan certainly believed in the logical character of his deductions, and we therefore criticise him mainly for his deficient knowledge of the "nature of things and men."

Ultrascientific critics like to remind us that cultural phenomena are very complex, that we are consequently not warranted in assuming more than a functional relationship between distinct traits. There is great merit in the contention, but like everything else it can be overdone. If South African natives and North American Indians respond to the impact of white civilization by quite similar Messianic cults, shall we not admit that the contact is a temporally antecedent cause? Or do the methodological wiseacres contend that the cults could bring about the invasion of the white race?

Morgan thus figures as a typical exponent of a contemporary philosophy of civilization. That philosophy was bedecked with the follies of fashion, but part of its core was sound and a discriminating analysis will try to preserve it. As an individual, Morgan was only moderately cultivated and indifferently familiar with culture history. Most emphatically, he was not, as he has been called, a man of "brilliant delusions." He had delusions, which he set forth with schoolmasterly pedantry; but he bril-

²³ Wilhelm Schmidt, "The Position of Women with Regard to Property in Primitive Society," *American Anthropologist*, XXXVII (1935), 244-256.

liantly illuminated the subject of kinship terminologies by immersing himself in the facts, persistently arranging them, seeking and rejecting specific solutions of the problems they presented. His was not a flashy intellect, but one of unusual honesty, depth, and tenacity; and his prolonged concentration achieved the triumph of glimpses of real insight in a virgin field of scholarship. There is no better example of Darwin's saying, "It's *dogged* does it."

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EVERY WELL-DEFINED SEGMENT OF REALITY CALLS FOR A branch of knowledge that shall record and interpret its phenomena. During the last hundred years it has become increasingly clear that culture—the sum total of socially acquired thoughts and practices—represents such a distinct domain; and the discipline dealing with it has been variously ticketed “culture history,” “ethnography,” “ethnology,” or “cultural anthropology.” This branch of learning does not confine itself to “primitive” tribes because the lines of cleavage between them and civilized peoples are arbitrary; also because a social tradition is equally distinctive of modern America and the Chinook Indians, of ancient Athens and the Australian aborigines. Whatever broader principles may emerge from a study of culture should therefore hold in some measure for all its levels.

The same subject matter can, of course, be approached from different and equally legitimate angles. Horses may be painted by a Rosa Bonheur or dissected by a comparative anatomist. So a culture may be described by a literary artist or by a

Read at the annual dinner of the Sigma Xi and Phi Beta Kappa societies of the University of Oregon, May 23, 1936, and printed in *American Journal of Sociology*, XLII (1936), 301–320.

scientist; their interests do not clash, because except by chance they do not meet. The artist's task is to convey an aesthetic impression, hence he rightly omits whatever might mar that effect; and he has no call to explain why the culture is what it is. The investigator, on principle, omits nothing because he ought to register the whole of his reality; and he must coördinate his data in spatial, temporal, and functional terms.

There is a widespread but, in my opinion, vicious tendency to dissociate social from natural science. On the one side we hear that the social sciences must consider values, hence subjective elements that militate against an objective approach. Again, their data are said to be so complex as to preclude generalizations. Some also stress the impossibility of experiment, the absence of mathematics.

Each of these objections rests on a misconception. Ethnology, for instance, must indeed deal with values, for they are part of its subject matter and to ignore them would be to neglect part of cultural reality. Thus, in Australia, as in many areas of Africa and America, a woman and her daughter's husband are forbidden to converse or even look at each other. The associated sentiments are part and parcel of the phenomenon. Translating into modern terms, we might easily view it in terms of our mother-in-law jokes. Nothing, however, would be farther from the truth, for wherever the aboriginal attitude has been determined it turns out to be one of mutual respect expressing itself in avoidance. This insight into the emotional states involved is, however, not the result of a magical empathy but rests on the utterances of native witnesses, which can be checked by further inquiry. Metaphysically it is of course conceivable that informants will lie about their sentiments; but, even so, if they all told the same story, such uniform prevarication would itself be a fact of importance. At all events, there is no other way of arriving at the truth in the matter than by questioning the natives and noting their behavior. Science determines subjective phenomena by objective methods.

Fortunately, human beings often voice their sentiments spontaneously, and in the collections of primitive prose now available there is abundant evidence as to the nature of personal relation-

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ships. One tale, for example, brings home the sacredness of comradeship when a father disowns his son for infidelity to his friend.¹ The traditions of several tribes incidentally demonstrate what has so often been denied—the occurrence of romantic love on the primitive plane. A Plains Indian lover is separated from his sweetheart. “As he sat there, he felt as though he must die of grief.” The girl “also was very heavy of heart.” She falls ill, and though the young man reaches her bedside she dies, leaving him as a souvenir a pair of beautiful moccasins embroidered by herself. “And the young man, caring nothing for the others who sat in the tipi looking at him, broke down and wept.” When the other people have moved on, he enters the tipi erected over her burial scaffold, and as a reward for his devotion she is allowed to revive and to marry him.² The theme of Orpheus and Eurydice, widespread in North America, gives similar evidence. To quote a Menomini version: “A certain man was married to a woman; greatly they loved each other. ‘If you die first, I shall go with you,’ he would say to that wife of his. And she, too, then would say the same to her husband.” The wife actually dies, and her husband promptly follows her to the hereafter. So in a central Californian variant the mourning husband goes to the grave and declares, “I’m going to stay here. I’m going to watch you. Where you’re going, I’m going.”³

Besides tales there are other sources for the true inwardness of native life. The autobiographies secured by Drs. Paul Radin, Julian H. Steward, and Truman Michelson contain priceless sidelights on native feeling and thought.⁴ The important point about both these personal narratives and the fictitious folk tales

¹ R. H. Lowie, *The Assiniboiné*, AMNH, Anthropological Papers, IV (1909), 206.

² Ella Deloria, *Dakota Texts* (New York, 1932), pp. 224–232.

³ Leonard Bloomfield, *Menomini Texts* (New York, 1928), p. 125; cf. A. H. Gayton, “The Orpheus Myth in North America,” *Journal of American Folk-Lore*, XLVIII (1935), 263–293.

⁴ Raul Radin, *Crashing Thunder* (New York, 1926); Truman Michelson, “Autobiography of a Fox Indian Woman,” BAE, 40th Ann. Rept. (1925), pp. 295–349; *idem*, “Narrative of an Arapaho Woman,” *American Anthropologist*, XXXV (1933), 595–610; *idem*, “The Narrative of a Southern Cheyenne Woman,” *Smithsonian Miscellaneous Collections*, LXXXVII, No. 5 (1932), 1–13; J. H. Steward, *Two Paiute Autobiographies*, Univ. Calif. Publ. Amer. Arch. and Ethn., XXXIII (1934), 423–438.

is that they yield objective evidence about subjective states. For what we want to ascertain is not how ethnologist X responds to the Winnebago scene, but what the Winnebago themselves feel; and with reference to their intimate life, that can be satisfactorily established only by recording their very words.

To prevent misunderstanding, I add that every addition to our stock of knowledge about aboriginal attitudes should be heartily welcomed. Thus, it is one of Boas' outstanding achievements to have directed attention to the primitive artist's psychological position toward his art. Not content to study museum specimens, Boas injected the question of how the innovator works on the basis of his tradition and how his environment receives his creations.⁵ But here once more the point is to obtain the *ipsissima verba* of the creators and of their critics. To depart from this ideal is to glide down the inclined plane of rank impressionism.

Other supposed reasons for an antithesis of anthropology and science rest on a misapprehension of natural science. Laymen readily hoodwink themselves into believing what no serious natural scientist claims, viz., that all natural science is concerned with Newtonian universals. Patently false for geology and the biological sciences, this proposition is not even true of physics, chemistry, and astronomy. Astronomers can predict solar eclipses, but they candidly admit their inability to predict meteoric showers. And the physicist, regardless of his lay touts, is usually satisfied with generalizations of moderate scope. When he is confronted with an irreducible fact, such as the expansion of water in freezing, he accepts it; he does not argue that it cannot be true, nor does he insist that it is worthless because other liquids behave differently. The position of the scientific anthropologist is precisely the physicist's and the astronomer's: he determines his data with the greatest precision feasible, generalizing about them exactly so far as the nature of the case permits and no further. As for experiments, the crucial experiment is not that in the laboratory but the *Gedankenex-*

⁵ Franz Boas, *Primitive Art* (Oslo, 1927), pp. 155 ff.; Franz Boas, H. K. Haeblerlin, J. A. Teit, and Helen H. Roberts, "Coiled Basketry in British Columbia and Surrounding Region," *BAE, 41st Ann. Rept.* (1928), pp. 119-484.

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periment, and that is quite as practicable in the field of culture as anywhere else. In short, cultural anthropology is simply science grappling objectively with one aspect of the universe and inevitably limited in its determinations only by the nature of its data—no more uniformly refractory than the astronomer's—and the provisional inadequacy of its present techniques. Its being in part historical does not make it antithetical to natural science, for geology, biology, and even astronomy are largely devoted to the study of sequences. The failure to employ mathematics on a major scale draws no line of cleavage, for here again there are natural sciences that use the calculus with equal restraint. Of organic chemistry, Professor Lewis declares: "The whole theory of structure requires about as much mathematics as a child needs for building houses with blocks."⁶

The first task of ethnology is to determine the spatial relations of all phenomena that are passed on from generation to generation by social inheritance. Nowadays it is fashionable to deride such investigations on the plea that they wrest an institution or industry from its natural setting. Each culture, it is averred, is a closed system to be viewed only as an integral whole.

As a matter of fact, this position was at one time opportune, when antiquarians were gleaning oddities of custom with the fervor of a philatelist collecting stamps. As a counterblast the totalitarian point of view was unquestionably helpful and timely, as proved by its many champions of otherwise diverse persuasion—men like Professors Boas, Radcliffe-Brown, Thurnwald, Malinowski, Durkheim. This relatively great service should not blind us to the doctrinaire nature of the propositions urged.

In the first place, a specific culture is an abstraction, an arbitrarily selected fragment. Social tradition in some measure varies from family to family. Shall we, then, study a single Hopi family, the Hopi village of Walpi or the seven Hopi villages, the Pueblo area of North America, if not the New World as contrasted with the Old? There is only one cultural reality that is not artificial, to wit: the culture of all humanity at all periods and in all places. The choice of a particular people is accord-

⁶ Gilbert N. Lewis, *The Anatomy of Science* (New Haven, 1926), p. 172.

ingly a limitation justifiable by expediency only; and there is no warrant for the a priori assumption that facts pertaining to Walpi are mystically connected only with other Walpi facts and with nothing else in the universe.

To be sure, bonds between intra-tribal elements have been established, for example, in the way of ceremonial patterns, but these have never been proved to cover more than a restricted portion of the empirically cohering features. Thus, the Arapaho, a Wyoming tribe, perform their several ceremonies only after a formal vow. This suggestive fact helps us understand why any new ceremony, whether borrowed or evolved within the tribe, would conform to the established standard. If this phenomenon, however, is somehow related to the taboo among the Arapaho against free chatting between brother and sister, the lack of clans, the smoking of catlinite pipes, and the use of snowshoes in buffalo-hunting, the correlation has never been demonstrated, and in my opinion it never will be.

The gist of the matter is simply this. Isolated facts yield no insight; but nothing warrants the assumption that all significant relationships are confined within the system of a particular social tradition. To wrest a fact from its tribal context is no more arbitrary than to wrest the tribe from its contacts with the rest of humanity; than to isolate the extremities of vertebrates for comparative anatomical study when we all know that the limbs never exist except in association with the organism as a whole.

A commonplace consideration may clinch the argument. The conception of the zero, as well as the arithmetical notation linked with it, has rightly been acclaimed as a capital achievement of the Maya mind. But what lends peculiar importance to the phenomenon? Not that the idea is imbedded in a ceremonial calendric system; irrational contexts of rational notions, once interesting, have become trite. But that the Indians of Yucatan were able to excel the Greeks and Romans—*that* is, indeed, a datum of primary significance, regardless of anything else we know about the Maya, and bearing in the most definitive manner on the racial potentialities of the Indian.

Years ago I wrote: "When we know only the range of a usage, we may not yet know very much, but we have at least a point

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of departure for amplifying our information. When we do not know the distribution of a phenomenon . . . , we know nothing that is theoretically significant." The statement was bitterly resented in some quarters, but the Maya case illuminates the intended meaning. Assume that the Australians and Andaman Islanders shared the position system with the Maya; obviously the fact would be seen in quite a different light. Or assume that the trait occurred generally among farming peoples but never in the pastoral or hunting condition; the import of the datum would again be altered.

The superb distribution studies of the late Baron Nordenskiöld and of Professor Leslie Spier are invaluable contributions to a science of culture not because knowledge of the geographical extent of traits is an end in itself but because accurate information on distributions raises problems of a basic character and helps toward their solution. Geographical considerations merge in those of a temporal and causal order. We may or we may not find an elusive all-integrating intra-tribal factor, but we must correlate significantly within and without the culture, and for that the distributions must be known. Why, for instance, is skin-dressing a masculine pursuit among our southwestern natives when elsewhere north of Mexico it is regularly a task for women? Spier notes that Pueblo men weave and plausibly suggests that skin preparation was accordingly transferred to them.⁷ Again, Nordenskiöld remarks that in South America the carrying-net or shawl occurs in the Inca empire while the Amazonian tribes use carrying-baskets. In the former, flour is prepared by grinding on stone hand-mills; the other substitutes pounding in a wooden mortar, etc.⁸ We know that many features have been diffused from Peru to adjacent territories; why have the Amazonians remained immune in the instances cited? Similarly, in the United States two forms of guessing-game, the moccasin and the hand-game, are in the main mutually exclusive—somewhat as in South Germany *tarok*

⁷ Leslie Spier, *Havasupai Ethnography*, AMNH, Anthropological Papers, XXIX (1928), 157.

⁸ Erland Nordenskiöld, *Comparative Ethnographical Studies* (Göteborg, 1930), VII, 134.

cards hold their own against the North German *skat*. Here is one of the ethnologist's perennial puzzles—the process of selective borrowing. What underlies it? Why will people take over only part of the alien traits to which they are exposed? Nordenskiöld gives a sound partial explanation: People do not supplant a well-tried device fulfilling a particular need. The distributional data, at all events, lead us into the very heart of our problems; imperceptibly we glide from “where?” to “why?”

Distributional phenomena lead to a study of man's adaptation to his physical environment. The extravagant claims of certain geographers—by no means all of them—have unduly prejudiced many ethnographers against the geographical point of view. Nothing is easier than to refute the notion that man automatically responds to a given practical problem in the most elegant fashion. In New Mexico and Arizona sedentary farmers living in stone houses have lived for centuries beside sparingly horticultural nomads building crude huts; and in the Gran Chaco the simpler Choroti inhabit the same territory as the Chiriguano, who are deeply tinctured with Inca influences. The Ona of Tierra del Fuego freeze in an Antarctic habitat because they have not contrived adequate dress or shelter, etc. Just as soon, however, as we cease to attach messianic notions to geography and treat it in a common-sense way, it becomes a helpful and necessary guide; and significant correlations emerge, partly on the intra-cultural plane, partly with outside factors.

The negative influence of environment has been generally recognized, but many overlook the positive complement of such inhibition. In the Chaco and on the upper Xingu the dearth of suitable material precludes a Stone Age. The aborigines, requiring some tools, substituted shell, bone, teeth, and hardwood, just as the Micronesians in a similar predicament made adze blades of the giant clam shell. What is more, geographical limitations are among the major motives for trade; and with commercial relations once established for a specific object, a contact is created that may lead to many other gains. Australians who go in search for diorite or the pitjuri stimulant also bring home songs, dances, and tales. In tropical South

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America the curare poison became an important article of trade. This led to a change of weapons in some regions where the blowgun would be ineffective in warfare and the hunting of large game; tipped with strychnine, its darts turned into deadly projectiles.⁹ Environment is thus no mean determinant of culture. We must beware of an intolerantly all-or-nothing position. Though geography is impotent to explain many phenomena we should like to understand, it is nevertheless capable of partly accounting for others, and for data admittedly complex we have no right to demand a single simple cause.

The distributional facts thus prompt basic problems as to why and how cultures differ, and also how one element in a geographico-ethnic situation may affect others. They further suggest questions of a historical order, i.e., of sequence and of tribal relations. Here we encounter a major objection, since some writers reject such matters as trivial. For scientific anthropology the rejoinder is simple. Since it embraces all the phenomena of culture in their varied relations, temporal succession is manifestly important because time is one of the inescapable categories of our thinking. We cannot picture a timeless culture any more than a spaceless one. The very occurrence of farming in contemporary France dates its culture, for in a Reindeer age it would have been impossible. Migration either of peoples or of technical processes is likewise inconceivable in certain areas during the boatless period of mankind unless there were land bridges, which again involves a geological time scale. As for tribal relations during the geologically Recent period, they are significant in several ways. Whether a parallel in two distant cultures is due to independent development or must have sprung from one source has a direct bearing on the nature of human or racial inventiveness—a question of manifest importance for an ultimate theory of culture. Further, since diffusion is an accepted fact, judgments varying merely as to its extent, we must amass all information we can about its conditions. From this angle the mere fact of dissemination is only a starting-point. We ought to ascertain who borrows from whom, noting the

⁹ Karl von den Steinen, *Unter den Naturvölkern Zentral-Brasiliens* (Berlin, 1897), pp. 196 ff.; Nordenskiöld, *op. cit.*, I (1919), 4, 15; III (1924), 4, 59 f.

comparative planes of the recipient and the donor; as already suggested, we must inquire why part of the possible features is rejected, and how the accepted offerings are assimilated. These are questions more easily asked than answered, yet an adequate theory cannot ignore them.

In the meantime we may record some gratifyingly solid results of historical studies. Archaeology, like paleontology, labors under certain limitations, but, like its sister-science, yields definite results. Metal ages do not occur everywhere in the same order, but everywhere metallurgy was preceded by work in stone, bone, wood, or shell. Hunting and gathering uniformly antedate farming. To be more specific, the ancient Peruvian coastal site of Paracas harbors only one variety of maize characterized by small dark-brown or black cobs; manioc and sweet-potato tubers found there are small and twisted, being similarly inferior to later samples.¹⁰ Here, as in the more familiar case of our southwestern Indians, we can thus follow the evolution of agriculture. Stratigraphic investigation has also repeatedly traced the succession of stone implements, of types of container, of pottery forms and pottery ornamentations. Even surface finds may suffice for establishing a sequence. In modern Amazonas, stone arrowheads are completely lacking, yet they are not rare in old settlements. The Indians evidently had stone implements when they entered this region, were unable to perpetuate stone work for want of raw material, and substituted bamboo or hardwood. Similarly, the Chaco tribes manufacture bone or wooden knives simulating the form of stone blades unearthed in archaeological sites farther west.¹¹

Naturally, sequences established without the aid of archaeology are generally less definitive, yet when worked out with due caution they may be accepted as contributions to knowledge. In any event they are not one whit more dubious than the comparable findings of natural science. Some of us are old enough to remember Lord Kelvin maintaining the physicist's point of view that the age of the earth was insufficient to allow

¹⁰ Nordenskiöld, *op. cit.*, IX (1931), 51.

¹¹ *Ibid.*, VIII (1930), 33 f.; cf. Sigvald Linné, *Darien in the Past* (Göteborg, 1929), pp. 54 f.

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for biological evolution. If modern estimates are right, the great British scientist was incomparably farther from the truth than the wildest speculators about culture history.

Nordenskiöld has drawn attention to an interesting distributional phenomenon. Comparing the two parts of the New World, he finds a large number of North American features recurring in South America, but only in the Chaco and regions south. More recently Krickeberg, without restricting himself to the tip of South America, has added appreciably to the parallels. Some of these, he believes, may have simply spread from tribe to tribe at various periods; others he ascribes, like Nordenskiöld, to a very ancient American layer, seeing that they are not found in the intervening areas.¹² This argument is indeed definitive, on two conditions. First, the features must not be lacking from mere failure on the part of the observers; second, they must be so well defined as to bar independent development. Neither Nordenskiöld nor Krickeberg seems to me to have sufficiently observed these precautions; but when due allowance is made, some striking parallels remain. Thus, a communal game drive by firing the grass is reported for the Algonkian and Siouan tribes of Wisconsin, Illinois, Iowa, North Dakota, and Minnesota; for some eastern and southeastern Indians of the United States; for the Havasupai of Arizona, the Coeur d'Alène of Idaho, and several North Californian tribes.¹³ According to Krickeberg, this practice was shared by the Brazilian Ge, the Abipone and Pilaga of the Chaco, and the Patagonians. This trait I should concur in assigning to a very old cultural layer, possibly a proto-American one, for we know that communal hunts are ancient and in such *battues* fire would be a most effective agent in driving large game toward a cliff or pound. Once invented, the method would not be readily abandoned. Obviously, however, the tropical forest does not provide a favorable terrain, nor would preponderantly horticultural peoples tend to per-

¹² Nordenskiöld, *op. cit.*, VIII (1930), 127 f.; IX (1931), 77-94; W. Krickeberg, "Beiträge zur Frage der alten kulturgeschichtlichen Beziehungen zwischen Nord- und Südamerika," *Zeitschrift für Ethnologie*, LXVI (1934), 287-373.

¹³ Clark Wissler, *Material Culture of the Blackfoot Indians*, AMNH, Anthropological Papers, V (1910), 51; Kaj Birket-Smith, *The Caribou Eskimos* (Copenhagen, 1929), II, 330 f.

petuate such drives. Thus, both the positive and the negative instances are satisfactorily subsumed under the hypothesis that the New World drives with grass-firing represent one historical phenomenon belonging to a very early hunting culture.

For corresponding reasons I share Nordenskiöld's view that the far-flung North American mode of boiling by dropping hot stones into a container of basketry, wood, hide, or bark passed into South America, surviving only among the Chono of Chiloe Island and the Kaingang of southeastern Brazil, with some suggestions of the practice in Tierra del Fuego.¹⁴

For a number of chronological determinations anthropology relies on zoölogical and botanical results. Thus, with some exceptions the American aborigines from Alaska to Tierra del Fuego have dogs. Have these canines been independently domesticated in the several regions from local forms of wolves, coyotes, or foxes? Or do they represent an import from the Old World? Comparative anatomists assure us that aboriginal dogs cannot be descended from American wild forms, but one and all go back to a single Asiatic wolf.¹⁵ By itself this does not exclude comparatively recent transmission from Asia, but such an assumption is impossible. In the first place, canine remains occur in archaeological sites of obviously considerable age. Second, notwithstanding their common origin, the pre-Columbian dogs of the continent represent some sixteen distinct breeds, differentiation indicating long domestication. Again, the beast occurs among the very simplest tribes and those most remote from the point of entry—such as the Basin Shoshoneans and the Fuegians.

Utilization of the species shows great variability. Dog traction is found with sledges among the Eskimo, with the travois or frame connecting dragging poles on the Plains, but is lacking farther south. The Dakota and Arapaho ate dogs, a custom positively abhorrent to the Yahi of northern California, who

¹⁴ Nordenskiöld, *op. cit.*, IX (1931), 86; Hermann Ploetz and A. Métraux, "La Civilisation matérielle et la vie sociale et religieuse des indiens Ze du Brésil méridional et oriental," *Revista del Instituto de Etnología de la Universidad Nacional de Tucumán*, I (1930), 170 f.

¹⁵ Glover M. Allen, "Dogs of the American Aborigines," *Bulletin of the Museum of Comparative Zoölogy, Harvard*, LXIII (1920), 431-517.

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considered the flesh deadly poison craved by evil sorcerers for their diabolical practices. These people hunted bear and deer with dogs; and to the Ona of Tierra del Fuego these animals are a virtual necessity for bringing down guanacos. In north-eastern Bolivia, on the other hand, dogs are of no practical importance. Emotional attitudes are equally variable. The Pauserna of Brazil starve their pariah dogs, while a Choroti woman in the Chaco will suckle a pup beside her own infant. In Arizona, again, the Maricopa treat dogs as persons, give them names, and dream about them.

Such biological and cultural differentiation then, supported by archaeological data, indicates that the dog belongs to a very ancient American stratum. Introduced by early immigrants from Asia, it gradually spread southward, developed local breeds, assumed varying uses in different regions, and stimulated distinctive attitudes in different areas.

The pan-American approach need not be limited to material culture. In comparing myths some investigators treat vague similarities as significant, but that is a peril of all scientific comparison. Obviously we must also guard against mistaking a post-Columbian parallel due to common Negro or European influences for resemblance proving ancient contact. A tale that has impressed me as really pertinent to the argument embodies the dualistic conception of mythical heroes, generally brothers, sometimes twins, one of whom endeavors to create for mankind a life eternal of ease and plenty but is thwarted by his marplot comrade. Gusinde has suggestively discussed the South American myths of this category, but without pointing out what is to me the most remarkable phenomenon, viz., the differential resemblances of the Yaghan and Ona versions with those from California and the Great Basin. Since these last two regions are geographically contiguous and demonstrably have close cultural relations, they form a unit as against the Fuegians.¹⁶

The resemblances are of a specific character—a conclusion

¹⁶ Martin Gusinde, "Das Brüderpaar in der südamerikanischen Mythologie," *XXIII International Congress of Americanists* (Lancaster, Pa., 1930), pp. 687–698; Wilhelm Koppers, *Unter Feuerland-Indianern* (Stuttgart, 1924), pp. 202–208; Alfred Métraux, *La Religion des Tupinamba* (Paris, 1928), pp. 31–43.

forcibly brought home by the much wider divergence between the Yaghan and the Tupi-Guarani twin stories than between the Yaghan and the Californian-Basin versions. Obviously it is not a matter of psychic or of pan-American unity. There is a differential resemblance, such as confronts the naturalist who finds, say, a marsupial in Australia and again in America. The ethnographer, of course, does not infer a direct genetic kinship between Fuegians and Californians but merely a pristine contact producing an exchange of myths. A historical relationship undocumented by direct evidence is indicated. To assign the myth itself to a definite period is far more difficult. I hesitate to place it in the proto-American layer, so long as it remains unknown from other equally rude peoples such as the Brazilian Ge. There is likewise the question of stability. How probable is it that a tale would retain essential elements for, say, five thousand years? Yet Boas has found that quite trivial stories, which at first blush would suggest narratives of personal adventures, turn up in identical form among widely and long separated branches of the Eskimo. This objection, therefore, is not a serious one. It could also be met by Nordenskiöld's view that the ancestors of the Fuegians traversed South America with great rapidity so as to remain virtually unaffected by the influence of the tropical territories passed. However, I cannot readily assume that Californians and proto-Yaghans lived in amicable neighborliness a few centuries ago. Both the "Canoe" and the "Foot" Indians of Tierra del Fuego show far too many adaptations to their present habitat to be credited with a really recent tenancy. Late diffusion being barred by the lack of the myth in intervening areas, I should put its origin somewhere in the era intermediate between the occupation of the Fuegian archipelago and the specialization of Californian culture.

The conclusion of historical relations between the Fuegians and North American Indians is strengthened by concomitant resemblances. Thus, Father Schmidt points out the rule by which initiates in California and in Tierra del Fuego must not scratch themselves with their fingers, for which a special little rod is substituted.¹⁷ This feature is shared by so many other American

¹⁷ Wilhelm Schmidt, *Ursprung der Gottesidee*, II (1928), 1023.

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groups that it does not by itself demonstrate specific relations between the two areas. Nevertheless, its occurrence indicates that the mythological parallel does not stand alone.

As a matter of fact, the use of a head-scratcher merits historical consideration in its own right.¹⁸ This quaint practice ranges from British Columbia to the very tip of South America. It is inconceivable that such a custom should have spread through recent propagandist fervor. Had such been the case, we should expect to find it in the same invariable setting, whereas actually the situations vary. Often it adheres to puberty initiation, but now to that of girls exclusively, now to the boys', and again to that of either sex. The implement may be associated with a tribal ceremony like the sun dance or with the homecoming of a defiled warrior, with funeral rites or with the birth of a child. Generalizing for the whole hemisphere, we might say that it potentially crops up in situations of ceremonial stress or seclusion, but obviously a custom is borrowed not in the abstract but in a concrete form. In view of the immense distribution of the scratcher, the variations in its correlates, and its occurrence among such unequivocally simple tribes as the Californians, the Ge, and the Fuegians, I think we can safely assign it to an extremely early layer of American culture, to the same period roughly as stone-boiling, to an earlier period than the Marplot theme.

Ethnology may thus engage in chronological hypotheses, which are not indeed experimentally verifiable, but, like the geologist's and zoölogist's historical conjectures, may become more probable in the light of extraneous facts. The stratigraphic findings of archaeology claim the validity of any direct scientific demonstration.

Science, however, takes cognizance not only of the categories of space and time but also of causality. I am acquainted with the philosophical problems connected with this matter since the days

¹⁸ Edward Sapir, "A Girl's Puberty Ceremony among the Nootka Indians," *Transactions of the Royal Society of Canada*, 3d ser., VII (1913), 79; Cora Du Bois, *Wintu Ethnography*, Univ. Calif. Publ. Amer. Arch. and Ethn., XXXVI (1935), 52; A. L. Kroeber, *Handbook of the Indians of California*, BAE, Bull. 78 (1925), p. 254; Leslie Spier, *Yuman Tribes of the Gila River* (Chicago, 1933), pp. 180, 325, 327; Th. Koch-Grünberg, *Vom Roroima zum Orinoco* (Stuttgart, 1923), III, 130, 168; communication from Curt Nimuendajú about the Timbira.

of Hume and Kant, and the attempts to supplant causality with the mathematical notion of function by Mach and Bertrand Russell are familiar to me. For my present purposes the distinction, however, seems unessential. The question is simply whether ethnology can give satisfactory reasons why cultural phenomena are as they are. To a certain extent the answer has already been given. Every ecological interpretation involves at least one definite determinant of the fact to be accounted for. The Chaco tribes ceased to make stone tools in their present habitat because they no longer had the raw material; Australians do not grind stone when the diorite suitable for such a technique is beyond reach, etc. Explanations of equal simplicity often emerge when the ramifying branches of a once united stock develop different traits. An oft-cited illustration is that of the Athabaskan family, divisible into one branch that extends from Alaska to Hudson Bay; another in and near northwesternmost California; and a third in New Mexico and Arizona. The Navaho of Arizona, for example, have quite obviously taken over features of Pueblo life such as maize cultivation, which clearly set them off from their congeners in the north; and the Hupa of California have become almost indistinguishable from their alien neighbors. Were it not for the testimony of language, indeed, no one would ever suspect either Navaho or Hupa of Athabaskan affinities. Similarly, the Chiriguana, who emigrated from Paraguay toward the Andean region in the sixteenth century, have adopted various features distinguishing them from their fellow-Guarani.¹⁹

Logically, this type of problem presents itself as follows. Group A, which once formed part of a major unit, has become detached and now differs from Groups B and C; how are the differences to be explained? If the remote segments came to vary in the same direction, we might think of a common law of development; since they diverge, the differences must be due either to independent evolution or to novel influences from new neighbors. In some instances, such as the adoption of a hitherto unknown plant not available in its wild form, only the second alternative is possible; and in many other cases it offers the only probable explanation. This is not evading the above-mentioned problem of selective

¹⁹ Nordenskiöld, *op. cit.*, VIII (1930), 157.

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borrowing. For, in our Southwest the Navaho or Maricopa have not taken over the architecture of the Pueblo tribes; while in the Chaco the Choroti and the Chiriguano represent a comparably lower and higher plane of living. Contact with new groups is no master-key for all hidden mysteries; suffice it that it solves definitely and definitively a particular set of problems.

Some minds, however, are not content with so specific a form of interpretation, but aspire to principles of absolute generality. Reverting to what I have already said, I insist that this ideal is only to a most moderate degree realized even in physics while the saner philosophers of science have increasingly deprecated possession of a Laplacean formula for all the past, present, and future happenings in the universe. In the study of culture we can certainly put forth universally valid propositions, but they are not particularly interesting. It is true that not a single hunting tribe has mastered analytical geometry, but somehow the statement, impeccable though it be, fails to shed illumination. On the other hand, the "law" once postulated that the rituals of hunters-gatherers revolve about food species is not a law at all. Unquestionably many ceremonials are devoted to insuring the food supply. But the buffalo-hunting Crow had as their main rituals the sun dance and the tobacco dance, the former pledged to secure vengeance, the latter for the generic benefit of the tribe. In part of California the mourning anniversary embodies the peak of native ritualism; in some other sections the girls' adolescence ceremony looms large. A real law would have to account for such deviations.

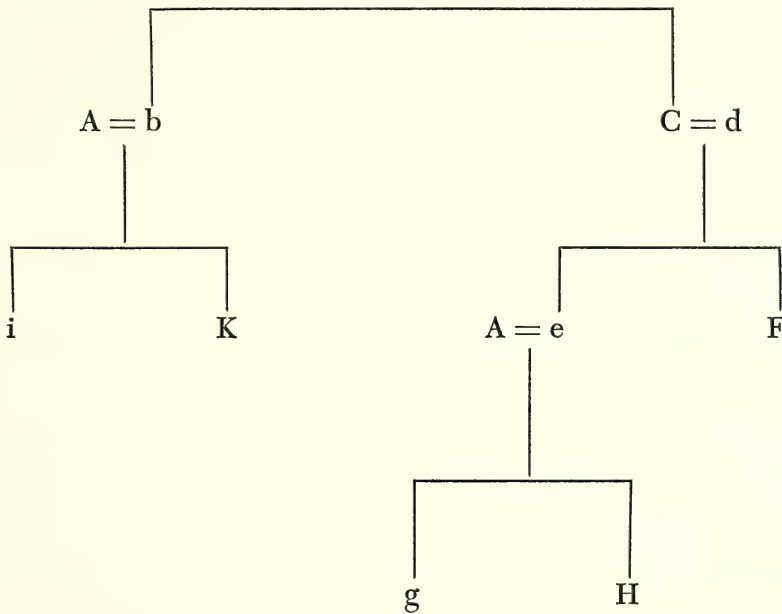
The social scientist who plays the sedulous ape to mechanics makes himself ridiculous at a time when the hegemony of mechanics is not even recognized in other departments of physics or chemistry. The social scientist, like the organic chemist, is not obliged to conform to a model set by an alien discipline; his business is to coördinate in consonance with the nature of his phenomena. Now to date these phenomena suggest to me no absolute laws; but they do point toward certain regularities, and these it is certainly our duty to ascertain as rigorously as possible.

In our central states a solid block of Algonkian (Menomini, etc.) and Siouan (Omaha, etc.) tribes share the so-called "Omaha type" of nomenclature, i.e., they call the son of a maternal uncle

by the same term as the uncle himself. In this respect they differ conspicuously from fellow-Algonkian and fellow-Siouan tribes, respectively. This divergence can, of course, be explained by borrowing along the lines indicated; if, say, the Algonkians were the first to adopt this mode of classing certain cousins, then their Siouan neighbors differ from other Siouans because of the Algonkian influence. But obviously the explanation cannot simultaneously serve for both deviating groups; so on this assumption it remains unexplained why the Algonkians of the area should have departed from prior Algonkian usage. To discover the determinants of the phenomenon discussed, we naturally inquire whether it occurs elsewhere; and, if so, with what concomitants. The geographically nearest parallel to the Omaha type exists among the Californian Miwok and their neighbors. Any historical relation between the Miwok and the Indians of our central states is out of the question; fifteen hundred miles apart, they are separated by tribes with no trace of an intermediate kinship system, without a suggestion of a linguistic affiliation with either Algonkian or Siouan. What, then, causes the strange duplication of failing to distinguish between two adjacent generations, between an uncle and a cousin? An obvious feature common to the central Algonkians, southern Siouans, and Miwok is their patrilineal clan system, for such an organization puts the uncle and nephew in question in the same clan, their classification under one head becoming intelligible: the clan bond overrides such factors as age and generation. But this is not an adequate explanation, because other patrilineal tribes, such as the Algonkian Ojibwa, lack the Omaha nomenclature, i.e., distinguish the avuncular-nepotic generations. If patrilineal descent is involved at all, then, it must be reinforced by a supplementary factor. Now, the Omaha and the Miwok share a marriage rule that tends to level the difference of generations in question. Both tribes permit a man to marry his wife's niece, i.e., her brother's daughter; the wife's niece is thereby equated with the wife, and so are their respective children, inasmuch as they are all equally the children of the same man. Let us, then, take the son of the niece in relation to that niece's brother; the older man is literally the maternal uncle of the younger. But the double marriage makes the first wife also a "mother" of her

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niece's children since she, too, is a wife of their father. Hence, the brother of the older wife is another "maternal uncle" in principle, while actually he is of course the father of the true uncle. Hence, the same person is simultaneously maternal uncle and maternal uncle's son to the offspring of the younger wife (his sister). The mysterious confounding of generations is explained by the sanctioning of simultaneous or successive marriage with a woman and her brother's daughter.



The provisional conclusion, then, is that the Omaha peculiarity is a function of patrilineal descent and a certain rule of marriage. It is quite possible that additional determinants must be brought in to explain all occurrences of the phenomenon. It is even possible that other determinants may yield the same result. What I am concerned with is the method of procedure, the recognition of how an explanation may be reached. That the same effect may be due to convergence is true and must be accepted as a fact wherever it is demonstrable. But this never explains the resemblance to be accounted for, since we possess no organ for grasping how unlike causes can yield like effects, only a principle of

causality by which the same causes are understood to produce like effects. We have the choice of accepting the incomprehensible with a shrug over the complexity of our data; or of seeking the uniform antecedents of similar facts. In the latter case we can proceed only by the approved processes of logic, the method of varying concomitants. Only by this procedure shall we be able to corroborate or repudiate the numerous correlations that are being constantly alleged and so rarely demonstrated. I am confident that from a thoroughgoing application of this method a number of significant regularities will emerge, representative of the type of generalization consistent with the nature of our field at the contemporary stage of insight. Our generalizations will not assume the form of a Laplacean formula but the form of propositions such as these: The Omaha nomenclature is a function of patrilineal descent when coupled with certain intergeneration marriages; the avunculate is a function of matrilineal residence coupled with such-and-such further conditions; handmade pottery is a woman's craft except when it becomes a specialized economic activity.

Ethnology will make immeasurable strides forward as soon as everyone who affirms correlations will favor his colleagues with a proof on an inductive basis.

To sum up: Ethnology is simply science grappling with the phenomena segregated from the remainder of the universe as "cultural." It is a wholly objective discipline, whether it deals with subjective attitudes or not, for its function is the determination of reality in verifiable terms. It coordinates its data spatially, in so far forth duplicating the procedure of geography. It coordinates its data chronologically to that extent sharing the logic of geology, paleontology, historical astronomy, and political history; the particular techniques employed must vary with the problem, as in other branches of learning. Finally, it coordinates in terms of causality as the concept has been epistemologically purified; and by the demonstration of functional relationships it may attain the degree of generalization consistent with its own section of the universe.

Evolution in Cultural Anthropology: A Reply to Leslie White

LESLIE WHITE'S LAST THREE ARTICLES IN THE *American Anthropologist*¹ require a reply since in my opinion they obscure vital issues. Grave matters, he clamors, are at stake. Obscurantists are plotting to defame Lewis H. Morgan and to undermine the theory of evolution.

Professor White should relax. There are no underground machinations. Evolution as a scientific doctrine—not as a farrago of immature metaphysical notions—is secure. Morgan's place in the history of anthropology will turn out to be what he deserves, for, as Dr. Johnson said, no man is ever written down except by himself. These articles by White raise important questions. As a victim of his polemical shafts I should like to clarify the issues involved. I premise that I am peculiarly fitted to enter sympathetically into my critic's frame of mind, for at one time I was

American Anthropologist, XLVIII (April-June, 1946), 223-233.

¹ Leslie A. White, "Energy and the Evolution of Culture," *American Anthropologist*, XLV (1943), 335-356; *idem*, "Morgan's Attitude toward Religion and Science," *ibid.*, XLVI (1944), 218-230; *idem*, "'Diffusion vs. Evolution': An Anti-Evolutionist Fallacy," *ibid.*, XLVII (1945), 339-356.

as devoted to Ernst Haeckel as White is to Morgan. Haeckel had solved the riddles of the universe for me.

ESTIMATES OF MORGAN

Considering the fate of many scientific men at the hands of their critics, it does not appear that Morgan has fared so badly. Americans bestowed on him the highest honors during his lifetime, eminent European scholars held him in esteem. Subsequently, as happens with most celebrities—Aristotle, Darwin, George Eliot, for example—the pendulum swung in the opposite direction. The reaction overshot its mark at times, as when Americanists doubted even Morgan's Crow findings. Nevertheless, appreciation has been frequent and ample even in later periods. Haddon calls Morgan "the greatest sociologist of the past century"; Rivers hails him as *the* discoverer of the classificatory system; Radcliffe-Brown rates *Systems of Consanguinity and Affinity* a monument of scholarly, patient research; Mitra pictures Morgan as an anthropological colossus, a greater Tylor; Marcel Mauss and Paul Radin are avowed admirers.² What, precisely, does White expect? An academic muezzin at every center of learning who shall lead anthropologists in daily Rochester-ward obeisances and genuflections?

MORGAN AND "BOASIANS"

The term "Boasian" is misleading. Of the great physiologist Johannes Müller a one-time disciple said: "There is no school in the sense of common dogmas, for he taught none, only a common method." This holds for Boas. His students have often differed from their teacher and from one another. Kroeber, Sapir, Radin have repeatedly expressed their dissent from cardinal "Boasian" views, and even I have uttered misgivings on certain points. Laufer incidentally, was not trained by Boas at all and was doubtless more deeply influenced by Eduard Hahn.

² Alfred C. Haddon, *History of Anthropology* (London, 1934), p. 127; W. H. R. Rivers, *Kinship and Social Organisation* (London, 1914), pp. 4 f.; A. R. Radcliffe-Brown, "The Study of Kinship Systems," *RAI, Journal*, LXXI (1941), 4 f.; Panchanan Mitra, *A History of American Anthropology* (Calcutta, 1933), pp. 109-120.

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Speaking *pro domo*, I find White's procedure curious. He virtually accuses me of plagiarizing Morgan on the subject of animal domestication³ when I merely state matters of long-established common knowledge. Morgan's reference, incidentally, is so casual as hardly to merit notice, and Francis Galton's full discussion of the point, which I have duly registered, is much earlier. Again, in White's latest article I am referred to fifteen times, but only three of the publications cited appeared after 1922, though I have twice dealt with Morgan rather fully in much later years.⁴ In these recent discussions, as in my *Primitive Society* long ago, I explicitly mention Morgan's use of diffusion,⁵ yet White finds it "difficult to see how Lowie could have read the passages in Morgan" concerned with that principle.

I do not, however, impugn White's good faith; the obsessive power of fanaticism unconsciously warps one's vision.

As a matter of fact, my conscience is clear on the subject of Morgan. In 1912, in the face of Americanist skepticism, I substantiated his discovery of matrilineal clans among the Crow. In 1916 I referred to his "superb pioneer achievements"; in 1917 I took pains to show that he was right and I wrong on an important point in Crow kinship nomenclature. In 1920 I called *Ancient Society* "an important pioneer effort by a man of estimable intelligence and exemplary industry"; in 1936 I commended his acuity as a field worker and credited his *Systems* with "a magnificent and valid conception."⁶

To be sure, eulogistic comments are balanced by harshly criti-

³ White, "Energy and the Evolution of Culture," p. 339.

⁴ R. H. Lowie, "Lewis H. Morgan in Historical Perspective," in *Essays in Anthropology Presented to A. L. Kroeber* (Berkeley, 1936), reprinted as No. 26 in the present volume; *idem*, *The History of Ethnological Theory* (New York, 1937).

⁵ *Idem*, *The History of Ethnological Theory*, p. 59; *idem*, *Primitive Society* (New York, 1937), p. 147.

⁶ *Idem*, *Social Life of the Crow Indians*, AMNH, Anthropological Papers, IX (1912), p. 186; *idem*, "Historical and Sociological Interpretations of Kinship Terminologies," in Holmes Anniversary Volume (Washington, 1916), p. 293 (reprinted as No. 3 in the present volume); *idem*, *Notes on the Social Organization and Customs of the Mandan, Hidatsa, and Crow Indians*, AMNH, Anthropological Papers, XXI (1917), 56; *idem*, *Primitive Society* (New York, 1920), p. v; *idem*, "Lewis H. Morgan in Historical Perspective," pp. 170, 180; *idem*, *The History of Ethnological Theory*, p. 92.

cal ones. As my treatment of Boas indicates,⁷ I am not an idolater. In proof of my malevolence White likes to quote a sentence dating back to 1920: "It may be said categorically that even at his worst Morgan never perpetrated more palpable nonsense, and that is saying a good deal."⁸ *More suo*, White fails to explain what evoked such violence. Since in 1877 there were already trustworthy reports on African Negroes and Polynesians, I regarded Morgan's denial of monarchic and aristocratic institutions among primitive peoples as inexcusable. Nowadays I should use more temperate phraseology, but as to the substance of my remark I remain adamant. Indeed, by way of amendment, I should say that Morgan is guilty of still less defensible propositions. How could any ethnographer ever put the Polynesians into the same category with Australians and *below* the Northern Athabaskans? Again, Morgan finds that "the discrepancies between them [Seneca and Dravidian kinship systems] are actually less . . . than between the Seneca and the Cayuga." This does not deter him from inferring a *racial* affinity between Dravidians and Seneca because of the identity of their kinship terminologies.⁹ The Seneca, by implication then, must be racially closer to the Tamil than to their fellow-Iroquois. Is this line of argument to be rated brilliant, profound, sensible, dubious, or is it palpable nonsense? How does Professor White grade it?

Professor White may say that we ought to judge a scholar by his positive contributions, and I heartily concur. Yet fairness to other scholars viewed in historical perspective demands that we should not gloss over such flagrant delinquencies, especially when their perpetrator regards them as cardinal discoveries. I should like to see some realization on White's part that sporadic impatience with Morgan may have an objective basis.

Certainly irritation at him is not necessarily bound up with anti-evolutionism. How, otherwise, does White explain the generally tender treatment of Tylor? I herewith offer some purely

⁷ *Idem*, *The History of Ethnological Theory*, pp. 151-155; *idem*, "Franz Boas, His Predecessors and His Contemporaries," *Science*, XCVII (1943), 202-203.

⁸ *Idem*, *Primitive Society*, p. 389.

⁹ L. H. Morgan, *Systems of Consanguinity and Affinity of the Human Family* (Washington, 1871), pp. 166, 508.

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personal remarks to explain why, notwithstanding my appreciation of certain aspects of Morgan's work, I cannot take kindly to him as a scientific personality. For one thing, I resent his dreary schematism; for another, I find little evidence in him for that sympathetic projection into alien mentality which anthropology is supposed to foster. He avoids one of the major departments of culture because "all primitive religions are grotesque and to some extent unintelligible."¹⁰ He dogmatizes thus on the subjective feelings of our indigenes: "The passion of *love* was unknown amongst the North American aborigines of pure blood. The fact is sufficiently established by their marriage customs. They were given in marriage without being consulted, and often to entire strangers."¹¹ Were, however, any doubt possible as to Morgan's narrow-mindedness, it is dispelled by White's admirable edition of his hero's travel notes. A few gems suffice for demonstration: The frescoes of Michael Angelo are "substantially absurd," the Sistine Chapel is "a poor specimen of a Pagan Temple." The Roman Carnival is "an unutterable piece of nonsense and levity," proving the frivolity of the population. Catholic ceremonial evokes "inexpressible disgust." The people of southern Italy "are utterly worthless," Italians in general "degraded beyond all other peoples called civilized." On the other hand we learn: "Our country is the favored and the blessed land. Our institutions are unrivalled and our people the most advanced in intelligence. . . ." ¹²

Tylor, I feel, could not have written in this strain. Probably White does not acquiesce in his idol's Bilboesque sentiments. If this is a correct surmise, why does he dissent from Radcliffe-Brown's apt characterization of Morgan's provincialism? ¹³ Why does he ascribe criticism of Morgan to anti-evolutionism or other sinister motives when such obvious reasons are at hand? Evolution has very little to do with the case.

¹⁰ *Idem*, *Ancient Society* (Kerr ed.; Chicago, 1877), p. 5.

¹¹ *Idem*, *Systems of Consanguinity*, p. 491.

¹² Leslie A. White, ed., *Extracts of Lewis Henry Morgan's European Travel Journal*, Rochester Historical Society Publications, XVI (1937), 285, 290, 303, 311, 315, 327.

¹³ *Idem*, "Morgan's Attitude toward Religion and Science," *American Anthropologist*, XLVI (1944), 218 f., 230.

ANTI-EVOLUTIONISM

As there is no Boasian sect, so there is no Boasian "reactionary philosophy of anti-evolution," nor a "philosophy of planless hodge-podge-ism."¹⁴ The former phrase naturally suggests the degeneration theories which Tylor refuted in *Primitive Culture*. Contrary to what might be regarded as the implications of White's phrases, Boas and his disciples nowhere question the established facts of prehistory (nor does anyone else), witness his inclusion of Nelson's section on "technological evolution" in *General Anthropology*.¹⁵

It is, indeed, not easy to discover the meaning of White's accusations. On the one hand, he tries to clear Morgan of *unilinear* evolutionism, which is the butt of Boas's strictures. If he were correct (see below), it would merely prove that Boas misunderstood Morgan, not that he had an anti-evolutionary philosophy. On the other hand, White summarizes evolutionary doctrine in his "Energy and the Evolution of Culture," laying down propositions which Boasians may find trite and futile, but which do not arouse the Bryanesque ardor imputed to them when evolution is propounded. Boasians do not deny that man requires food, controls his environment with the aid of tools, improves his control by invention and discovery, and alters social structure as a result of technological evolution. I refer White to a paragraph of Boas's in his general text.¹⁶

As a matter of fact, no reputable scholar challenges either the demonstrable findings of prehistory or the economic truisms proclaimed by White, least of all, the Austrian school whose writings are evidently on his *Index librorum prohibitorum* since he contents himself with a garbled sentence borrowed from Kluckhohn concerning their views. It is, however, patent that Fathers Schmidt and Koppers are not anti-evolutionists in White's esoteric sense. They do not, to be sure, like the word "Evolution," but the reality they fully recognize. As Morgan generally speaks about

¹⁴ *Idem*, "Energy and the Evolution of Culture," p. 355; *idem*, "Diffusion vs. Evolution," p. 354.

¹⁵ Franz Boas, ed., *General Anthropology* (New York, 1938), pp. 150 ff.

¹⁶ White, "Energy and the Evolution of Culture," p. 354; Boas, *op. cit.*, pp. 678 f.

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"development," so they have plenty to say concerning *Entwicklung*, *Fortentwicklungen*, *Weiterbildungen*; and when Father Schmidt somewhere traces *Stufen der ganzen Entwicklung*, what are these but evolutionary stages? Such section headings as "*Der Schritt vom niederen zum höheren Jägertum*" or "*Von der Jagd zur Tierzucht*" indicate that an evolution of some sort is definitely assumed within the several *Kulturkreise*. It is *unilinear* evolution that the Austrians and the Boasians reject, but since White has latterly discovered that Tylor and Morgan are not unilinear evolutionists at all ¹⁷—a matter to be discussed below—what is the row about from White's point of view? It would seem that then "he is right and we are right, and all's as well as well can be." If he deigned to read the Catholic scholars he so airily dismisses, he would discover that in the field of empirical inquiry (as opposed to metaphysics) they are as technologically oriented as himself. Stressing economic conditions as *von ganz hervorragender Bedeutung*, they explicitly accept historical materialism as an excellent and even indispensable heuristic principle.¹⁸

Thus White's gloomy picture of most contemporary anthropologists plunged into Cimmerian darkness, unrelieved by a single lambent ray of evolution, is preposterous. He ought to realize that Thurnwald, Radcliffe-Brown, Radin, Lesser, Malinowski are professed evolutionists, and that even I have spoken kindly enough of neo-evolutionism.¹⁹

The questions which worry White, viz., "why Boas and his disciples have been anti-evolutionists" and what may be "the source and basis of the anti-evolutionist philosophy of the Boas group," automatically disappear. In order to infuse sense into such queries they must be re-formulated: Why have Boas and his students attacked not evolution, but Morgan's and other writers' evolutionary schemes?

Characteristically White does not attempt to answer the ques-

¹⁷ White, "Diffusion vs. Evolution," p. 347.

¹⁸ Wilhelm Schmidt and Wilhelm Koppers, *Völker und Kulturen* (Regensburg, 1924), pp. 382, 396 ff., 625 ff., 636.

¹⁹ Lowie, *The History of Ethnological Theory*, pp. 246, 289; Bronislaw Malinowski, *A Scientific Theory of Culture and Other Essays* (Chapel Hill, 1944), p. 17; Richard Thurnwald, *Die menschliche Gesellschaft* (Berlin and Leipzig, 1931-1935), I, 16.

tion genetically. He makes a great to-do about Morgan's never alleging the priority of animal husbandry to agriculture (as if anyone had made the charge); he triumphantly points to Morgan's placing pictography before the alphabet (a matter not in dispute). But he preserves a discreet silence on virtually all matters that are relevant to the debate.

Boas began as a unilinear evolutionist. In 1888 he defended "the current view of a necessary precedence of matrilineal forms of family organization."²⁰ I also recall his telling me how deeply he was impressed by a first reading of Tylor's "adhesion" study. All problems then appeared solved, at least in principal. What was it, then, that made him alter his convictions? That is a worthwhile psychological problem.

Before directly answering this question it is well to digress and take up certain startling discoveries announced in White's second article, for they explain in part why he does not understand what the discussion is about.

NOVEL DISCOVERIES IN THE HISTORY OF SCIENCE

Between 1859 and 1881, we learn, thinking people were divided into two hostile camps: they championed either science or theology. A devout Christian could not be a scientist or a Darwinian. "*Those who opposed Darwinism did not labor for, or make contributions to, science. . . . If you were for Theology, you were against Science.*"²¹

This statement happens to be wrong in every particular and from every conceivable angle. Darwinism and science never have been interchangeable terms. Pious Catholics, witness Pasteur and Mendel, made epoch-making researches during Morgan's lifetime; Clerk Maxwell and Kelvin were, I believe, devout Protestants; Julius Robert Mayer was beyond any doubt a deeply religious man—and an opponent of Darwinism.²² The critics of Darwinism included towering figures in the history of science—Karl Ernst von Baer, Louis Agassiz, Rudolf Virchow, Albert Köl-

²⁰ Franz Boas, *Race, Language and Culture* (New York, 1940), p. 635.

²¹ White, "Morgan's Attitude," p. 219; the italics are White's.

²² Wilhelm Ostwald, *Grosse Männer* (Leipzig, 1910), pp. 73, 82.

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liker, Sir Richard Owen. Some of them were religious, others mere skeptics. On the other hand, Christian believers by no means uniformly rejected evolution; they included the geologist Joseph Le Conte and the botanist Asa Gray, whom Darwin held in high esteem.

The Manichaeon picture of Darwinian atheists as angels of light pitted against a Satanic brood of Christian obscurantists is merely more of Whitean melodrama. As might be expected, so revolutionary a doctrine as Darwinism evoked a variety of responses. Darwin's, Wallace's, Huxley's correspondence and Haeckel's polemical writings furnish ample illustrations. True scientists were thrilled over having so many obscurities illuminated for the first time. This group, as noted, comprised Christians, who promptly set about to harmonize their religion with their scientific convictions, revising what seemed unessential articles of faith. Materialists and other radicals just as naturally used the new ideas as grist for their mill, yet not all of them did so indiscriminately. David Friedrich Strauss, e.g., hailed Darwinism as a great achievement, yet found it still "highly imperfect." The inadequacies felt even by so sympathetic a scholar were naturally aggrandized and stressed by fundamentalists. Thus, there was by no means a clear-cut division into two hostile camps.

For present purposes we are interested in the *scientific* opposition. Why, we ask, did a man like Virchow maintain so reserved and at times hostile an attitude towards Darwinism? Why, half a century later, did experimentalists like Thomas Hunt Morgan remain critical? Whence the skepticism of Jacques Loeb,²³ whom even White can hardly brand as a fundamentalist? Were all these men reactionary philosophers, enemies of Science? The answer is clear. They objected to evolutionary theories on the ground that they were not scientific enough. Some of them cannot, nevertheless, qualify as "anti-evolutionists," for they gratefully recognized the widening of the intellectual horizon due to Darwin. But they rejected phylogenetic speculation, for which they sought to substitute the demonstrable findings of the laboratory.

²³ Jacques Loeb, in Heinrich Schmidt, ed., *Was wir Ernst Haeckel verdanken* (Leipzig, 1914), II, 15.

DIFFUSION AND EVOLUTION

Reverting to Boas, his critique of evolutionary schemes is the psychological equivalent of the experimentalists' critique of "the biogenetic law." The facts did not fit the theory, hence the theory would have to be modified or discarded. To cite concrete instances, L. H. Morgan teaches that the individual family is an end-product, preceded by various stages including that of a clan organization; Morgan, Bachofen, and Tylor teach the priority of matrilineal descent. Boas found that in the interior of British Columbia clanless tribes with a family organization and a patrilineal trend adopted from coastal neighbors a matrilineal clan organization.²⁴ Diffusion thus disproved the universal validity of the formula that Boas himself had been defending in 1888.

Subsequently it turned out that borrowing had played a far greater part among primitive groups than most anthropologists had supposed. The difficulty of establishing universal laws of sequence seemed correspondingly increased. In this sense I came to maintain that diffusion laid the axe to the root of any theory of historical laws.

Professor White in dissent blares forth a sennet of defiance: (1) Tylor and Morgan both accept diffusion in concrete cases, hence Diffusion and Evolution lie together as the leopard and the kid in Isaiah's peaceful kingdom. (2) Tylor "does not state, nor do his remarks imply or even allow of the intimation 'that every people must pass through all the stages of development,' as Boas claims." Similarly does Morgan nowhere "declare or even imply that each tribe, everywhere, must go through the same stages of cultural development."²⁵ (3) The Boasians fail to discriminate between cultural evolution and the culture history of specific tribes or peoples; Tylor and Morgan are never concerned with the history of tribes or peoples, only with that of cultural traits or complexes, such as writing, metallurgy, social organization. Hence the

²⁴ Franz Boas, *Die Resultate der Jesup-Expedition* (Separat-Abdruck aus den Verhandlungen des XVI Internationalen Amerikanisten-Kongresses, Wien, 1909), p. 16; John R. Swanton, "A Reconstruction of the Theory of Social Organization," *Boas Anniversary Volume* (New York, 1906), p. 173.

²⁵ White, "Diffusion vs. Evolution," p. 347.

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criticism that their formulae do not fit particular tribes is irrelevant, they were never meant to do so.

Let us scrutinize these allegations. That Tylor and Morgan knew about diffusion is, indeed, a patent fact, which I have taught for over twenty-five years, as White admits with reference to Tylor.²⁶ The point is irrelevant, for it is failure to integrate diffusion with evolution that is charged. To repeat a twice-told tale, Tylor offers the formula: (1) patrilocal residence; (2) taboo between wife and husband's kin. If the correlation in a dozen cases evolved independently, there is presumably an organic tie-up. But what if there has been a single historic center of origin for the trait couple? As a transcendent entity there might still be a law of sequence, but there would be no way of demonstrating it. Similarly with Morgan. If clans arise independently the world over out of similar antecedents, the clan may reasonably be put into a definite place in a chronological series. Not so if, as Morgan argues, it sprang up a single time and was thus diffused over the globe. Metaphysically, here also the sequence postulated is conceivable; empirically, however, it ceases to be demonstrable. To amend White's phraseology, "diffusion negates [the possibility of proving] evolution."

The criticism is that Tylor and Morgan fail to resolve this logical difficulty, not that they ascribed an independent development of Christianity to the Seneca or believed in an independent origin of maize-growing in the Balkans.

Contrary to White's allegations, moreover, Tylor and Morgan much more than *imply* a faith in parallelism. Both strongly believed in psychic unity. In accepting this principle Morgan declares: "It was in virtue of this that mankind were able to produce in similar conditions the same implements and utensils, the same inventions, and to develop similar institutions from the same original germs of thought." And in one of his most famous publications Tylor, having likened human institutions to stratified rocks, thus continues: "They succeed each other in series substantially uniform over the globe independent of what seem the compara-

²⁶ Lowie, *Primitive Society*, p. 147; *idem*, "Lewis H. Morgan in Historical Perspective," p. 173.

tively superficial differences of race and language, but shaped by similar human nature. . . ." ²⁷ Now, this last statement, to be sure, says nothing about specific peoples. The important thing, however, is not how a writer formulates his principles in the abstract but what he does with them.

Now, White to the contrary notwithstanding, Tylor and Morgan both apply their magical formulae to particular peoples. Tylor correlates cross-cousin marriage with exogamy. His schedules listed 21 peoples as practising cross-cousin marriage, only 15 of whom were described as exogamous. Sure of the general validity of the formula, he boldly places the remaining six tribes in the exogamous category.

Morgan finds Ponca, Winnebago, Ojibwa, and Menomini with patrilineal institutions, but from his formula he infers that they were once matrilineal. Similarly with the Greeks, despite "the absence of direct proof of ancient descent in the female line." Correspondingly, Greeks, Romans, Celts, Germans and Hebrews are credited with a pristine punaluan stage.²⁸

I suggest that this is no longer dealing with the cultural process, it is veritably "psuedo-history." Morgan is speaking of the past of Siouan and Algonkian *tribes*, and he defines it not on anything known about Siouan and Algonkian history, but as a deduction from his formula. And when a punaluan stage is ascribed to the remote ancestors of Greeks and Hebrews, it can be only on the assumption that these *peoples* have passed through the same stages as other peoples. Morgan does not ask in how far unique happenings in the past—say, alien influences—might have deflected these peoples from their predestined path.

Incidentally, White nowhere explains how he supposes the formulae to have been ultimately derived. Are they empirical inductions? In that case they must rest on observations of the history of specific tribes. Or are they all a priori constructs like the precious notion about the uncertainty of fatherhood as the cause of matrilineal reckoning in early times?

²⁷ Morgan, *Ancient Society* (Kerr ed.), p. 562; E. B. Tylor, "On a Method of Investigation of the Development of Institutions; Applied to Laws of Marriage and Descent," *RAI, Journal*, XVIII (1889), 245-269.

²⁸ Tylor, *op. cit.*; Morgan, *Ancient Society*, pp. 48 f., 59, 159, 161, 170, 175, 357, 388 f., 438.

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CONCLUSION

Leslie White misunderstands the status of the problem. It is false that any reputable anthropologist nowadays professes an anti-evolutionist philosophy in the sense alleged. The "anti-evolutionism" of the Boasians and of the Kulturkreislehrer has nothing to do with, say, the degeneration theory of de Maistre. It implies bad faith or bigotry to suppose that either Boas or Schmidt denies the findings of prehistory concerning the sequence of stone and metal tools, of simpler and of more complex economic systems.

But the Boasians do claim the right to check evolutionary generalizations by the facts they are meant to explain, precisely as the experimental zoölogists checked the recapitulation theory. A dogmatist naturally cannot understand that true scientists are not interested in proving a preconceived system. There are such. Psychologically viewed, Boas's attitude is simply that of Virchow towards Haeckel's phylogenetic hypotheses, that of Loeb in preferring demonstrable truths to fictitious genealogical trees. It is of a piece with Newton's "*Hypotheses non fingo*"; with Mach's aim to purge science of metaphysics; with Virchow's, Ostwald's, Mach's prescription to observe without preconceptions.

In Virchow's valedictory speech at Würzburg in 1856, so young Haeckel reported to his parents, the great pathologist explained to the students that his whole life's aim was devoted to discovering the unvarnished truth, to recognize it free from bias and to disseminate it unaltered. He exhorted them to get rid of all prejudices, "with which we are unfortunately crammed full from infancy on" (*mit denen wir leider von Kind auf an so vollgepfropft werden*) and to view things as they really are ("*die Dinge so einfach und natürlich anzusehen, wie sie sind*").²⁹ Some years previously, in one of his most famous papers, Virchow had already warned against the dangers that lurk in any system:

"Dann kommt jeden Augenblick der Konflikt zwischen dem System und dem einzelnen Fall, und gewöhnlich wird der einzelne Fall dem System geopfert."³⁰

²⁹ Ernst Haeckel, *Entwicklungsgeschichte einer Jugend* (Leipzig, 1921), pp. 200 f.

³⁰ Rudolph Virchow, *Die Einheitsbestrebungen in der wissenschaftlichen Medicin* (Berlin, 1849), p. 18.

Boas, too, refuses to sacrifice individual observations to a pre-conceived scheme, *voilà tout*.

In conclusion I reiterate that I am altogether convinced of White's good faith. But as Voltaire explains:

"La chose la plus rare est de joindre a raison avec L'enthousiasme: la raison consiste à voir toujours les choses comme elles sont. Celui qui dans l'ivresse voit les objets doubles est alors privé de la raison."

POSTSCRIPT

I have just encountered inexplicably forgotten passages in Father Schmidt's *Handbuch der Methode der kulturhistorischen Ethnologie* (Münster, 1937), which demonstrate beyond a doubt that he accepts not only the concept, but even the term "evolution":

Nun, Professor Lowie und andere werden aus meiner Darstellung der Methode ersehen, dass ich nicht nur das Wort, sondern auch den Begriff und die Tatsache der Evolution nicht vermeide, sondern, mit der ganzen kulturhistorischen Schule, frank und frei mich zur *Evolution* bekenne, aber nach wie vor in der Ablehnung des *Evolutionismus* verharre (p. vii).

. . . wer den Evolutionismus bekämpft und ablehnt, bekämpft und verwirft damit nicht die Evolution, die (innere) Entwicklung (p. 10).

The case against White is thus even stronger than previously indicated.

Franz Boas, 1858-1942

FRANZ BOAS, FOR MANY YEARS THE UNDISPUTED DEAN OF American anthropologists,¹ was born in Minden, Westphalia, on July 9, 1858. The son of educated parents in easy circumstances, he enjoyed standard preparatory instruction; and to the high ethical teaching imbibed in the household he referred feelingly in an open letter to President Von Hindenburg (March 27, 1933).

Entering the University of Heidelberg in 1877, he later shifted to Bonn and ultimately to Kiel, where he took his Ph.D. in 1881. Though his major interests then lay in physics and geography—his dissertation dealt with the recognition of the color of water—, his principal professor, Theobald Fischer, directed him also towards the historical and ethnographic aspects of geography. Through the mathematical training acquired during his university days Boas was subsequently able to follow, critically and constructively,

National Academy of Sciences, *Biographical Memoirs*, XXIV, No. 9 (1947), 303–320.

¹ The American Anthropological Association has issued an obituary memoir (Memor 61, 1943), which contains a complete bibliography and articles on Boas as a man by A. L. Kroeber, as an ethnologist by Ruth Benedict, as a linguist by M. B. Emeneau, as a physical anthropologist by Melville J. Herskovits, as a folklorist by Gladys A. Reichard, and as an archeologist by J. Alden Mason. I have drawn upon this publication, especially on Kroeber's biographical essay.

the rise of biometrics and its anthropological applications. But he did not narrowly specialize. For example, he read Gustav Theodor Fechner, including the delightfully humorous *Vier Paradoxa*. From his son-in-law, Dr. Cecil Yampolsky, I learn that Boas's letters of this period have been carefully preserved and that they reveal the nascent investigator's ardor for research. Publication of the correspondence would be a great boon, for it is likely to reveal intimate glimpses of the writer's personality, such as are all too rarely vouchsafed by his monographs and books.

Two years after the doctorate came the crucial expedition to Baffinland, ostensibly in the interests of geographical exploration, but ushering in a new era in Boas's life and in the history of Eskimo ethnography. Homeward bound, Boas paid his first visit to the United States and to New York. On his return to Germany he attached himself as an assistant to the *Königliches Museum für Völkerkunde* in Berlin, the institution founded and headed by Adolph Bastian; and in 1886 he received permission to lecture at the University as a docent. Doubtless he regularly attended the Berlin Society for Anthropology, Ethnology, and Prehistory, meeting its ruling spirit, the great pathologist Rudolf Virchow, to whom years later Boas paid a glowing obituary tribute.² It would be most interesting to know how these congenial intellects interacted, but nothing specific on their relationship is known. The obvious similarities between the two men have been repeatedly noted—their keen, analytical powers, their exceptional capacities for varied work, their independence, moral courage, and alert social consciousness. Similarly, it would be worth knowing to what extent Bastian influenced the younger man. Conceivably Boas's insistence on definite proof of cultural diffusion goes back to this source, but it is quite as plausibly explained in terms of Boas's own mentality. What personal intercourse with the older man doubtless did provide was an intimate knowledge of Bastian's theoretical views, often veiled for the mere reader by the most crabbed of styles.

The expedition to Baffinland yielded a number of papers, both popular and technical, on the region, the ethnographic publica-

² *Science*, n.s., XVI (1902), 441–445.

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tions culminating in the monograph on “The Central Eskimo.”³ In the meantime Boas found a new realm to conquer. A party of Bella Coola Indians exhibited in Berlin and the ample collections of the Museum stimulated an interest in northwestern North America. Boas pumped the natives for linguistic information, published the data secured, and in 1886 himself set forth for the coast of British Columbia. Thus started a notable research programme that occupied him literally until his death.

Returning to New York in 1887, Boas accepted a position as Assistant Editor of *Science* and married Marie Krackowizer, the daughter of an Austrian physician and forty-eightier who had gained distinction in America both as a medical man and a political reformer. Henceforth the United States became Boas’s home.

The British Association for the Advancement of Science had created a committee for the study of the tribes of British Columbia. From 1888 on, during Edward B. Tylor’s chairmanship, Boas repeatedly revisited the Northwest coast under this body’s auspices. His early reports bear witness to the range of his interests, which took in not only ethnography, but also linguistics and somatology. Sometime during these years Boas visited Tylor and Francis Galton in England, men for whom he retained a profound respect, which *more suo* did not preclude critical dissent. Here again it would be instructive to learn more about the measure of their direct influence. Tylor’s famous paper on the application of statistics to sociological problems (1889) certainly impressed Boas; for a while, he told me, it seemed as though everything could be solved by the methods there outlined. Galton he regarded as the true father of biometrics, for which Karl Pearson had furnished the technical apparatus. He recognized, of course, Pearson’s exceptional ability and once tried to visit him in England; but Pearson, though he had referred very cordially to Boas in the second edition of *The Grammar of Science*, for some reason declined to see him.

In 1888 Boas accepted a docentship at Clark University, remaining there until 1892, when he had his and America’s first anthropological Ph.D. student, A. F. Chamberlain. He left Clark

³ BAE, 6th Ann. Rept. (1888).

to become F. W. Putnam's chief assistant at the anthropological exhibits of the Chicago World's Fair, the core of the subsequent Field (Columbian) Museum. At this new institution he served as curator of anthropology, but was superseded by William H. Holmes. A year or two later he accepted an assistant curatorship under Putnam at the American Museum of Natural History, a position soon combined with a lectureship at Columbia. At that time this institution offered anthropological work under several distinct auspices, Ripley of *The Races of Europe* fame lecturing on that subject in the department of economics, while Livingston Farrand held forth on comparative sociology, religion, and art in the department of psychology. In 1889, however, Boas was appointed to head a new department of anthropology, with Farrand as his adjunct. Two years later he also became Putnam's successor at the American Museum.

His dual responsibility enabled Boas to bring students into contact with anthropological collections and, above all, to provide them with opportunities for field work under the auspices of the Museum. During this period developed the most ambitious research project of his career, the Morris K. Jesup Expedition, actually a series of expeditions designed to shed light on Asiatic-American relationships. Boas's collaborators included Farrand, Harlan I. Smith, and other Americans, as well as several noted European scholars, such as Waldemar Bogoras, Waldemar Jochelson, and Berthold Laufer. In this connection and later Boas evinced a rare capacity for enlisting the coöperation of men qualified to advance science. It was during his curatorship, too, that Roland B. Dixon, assisted by A. M. Tozzer, undertook the first strictly scientific investigation of a Californian tribe, culminating in the model monograph on *The Northern Maidu*. Even unacademic men—intelligent whalers, such as Captains Mutch and Comer—were drafted to make systematic observations on the Central Eskimo.

Several students, subsequently distinguished in the science, won their ethnographer's spurs under Boas's jointly curatorial and professorial tutelage—A. L. Kroeber, Clark Wissler, William Jones. Another fruitful institutional connection resulted from Boas's appointment (1901) as Honorary Philologist of the Bureau of Ameri-

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can Ethnology. It facilitated the accumulation and ultimate publication of vast bodies of linguistic material, as evidenced in the *Handbook of American Indian Languages* (1911, 1922).

A clash with Dr. Hermon C. Bumpus, then director of the American Museum, concerning methods of installation and the generic issue of departmental autonomy, led to Boas's resignation (1905) as curator and for many years severed the intimate bonds of the Museum department with that at Columbia. However, he soon found other outlets for his surplus energy. In 1908 he became editor of the *Journal of American Folk-Lore*; in 1910 he helped create the International School of American Archaeology and Ethnology in Mexico; in 1917 he founded the *International Journal of American Linguistics*; and for many years he edited the *Publications of the American Ethnological Society*. In 1908, moreover, the United States Immigration Commission authorized him to undertake a somatological study of European immigrants. The task once more involved the careful planning of a large-scale project with the aid of many assistants. Nor did personal field work cease: he directed excavations in Mexico and Porto Rico, went to the Kootenay and to the Keresan Indians, even revisited the Kwakiutl in his old age. Besides all this he regularly attended scientific congresses in America and Europe.

Boas's many-sided scientific activities found national and international recognition. He was elected to the National Academy of Sciences in April, 1900; was a member of the American Philosophical Society; president of the American Association for the Advancement of Science in 1931 and of the New York Academy of Sciences in 1910. Among his foreign honors may be mentioned the doctorate bestowed by Oxford University.

World War I and its aftermath brought to the fore some little-suspected facets of Boas's personality. He had long acquired American citizenship, but like many others found himself beset by a conflict of emotions. He was an internationalist if ever there was one; but he was also steeped in the culture of his native land, had close relatives living there, was linked by personal and professional ties with innumerable Germans. What is more, he had been in his 'teens when the millennial dream of a united *Reich* had come true; had lived through a period of spectacular positive

achievement in Germany. His attitude could not well be that of the 'forty-eight immigrants. He himself was aware of the difference and—probably thinking of his uncle, Dr. A. Jacobi—alluded in conversation to this disagreement between the two generations of German-Americans.

Feeling strongly, as always, on matters of principle, he bitterly resented the pro-Ally attitude of Americans as a breach of neutrality. "Oh, if we had a Grover Cleveland in the White House!" he exclaimed once, presumably referring to the Venezuelan episode. His was not a temperament that could restrain utterance in such a crisis. He wrote letters to the *New York Times* and to *The Nation*; contributed articles to *The Dial* and the *Illinois Staatszeitung*; preached a sermon on internationalism at St. Clark's Church; and parried a move to investigate the loyalty of the Columbia faculty by reading to a class his ideas on patriotism.

To be sure, only misunderstanding or malice could construe his stand as nationalistically pro-Kaiser. His posthumously issued pronouncements dating back to the period⁴ read very well in 1946. But thirty years ago they jarred upon people on the verge of war or actually embattled. Such auditors did not like to hear that their individualism and democracy, rooted in local conditions, were not necessarily superior to a polity like Germany's with her very different history. Still less did they thrill to the idea that obligations to mankind ought to take precedence of patriotism; nor were their susceptibilities assuaged by the admission that patriots—like the witch-hunters of an earlier period—might be utterly sincere and morally pure. Boas, however, pursued his way, unmindful of general unpopularity and the threat of academic discipline.

Post-bellum days saw him at the unprovocative but equally novel task of organizing the Emergency Society for German and Austrian Science. The man who had hitherto begrudged every minute of social life as an encroachment on professional work now lavished precious hours on routine jobs, on correspondence, on the search for new contacts that might aid in restoring the imperiled life of German science. This labor of love and self-abnegation was duly appreciated by its beneficiaries. When he applied

⁴ *Race and Democratic Society* (New York, 1945).

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for a visa in 1924, the German Consul at New York declined the customary fee. At the International Americanists' Congress held in Hamburg in 1930 Professor Sapper conveyed to Boas a diploma of honorary membership in the Geographical Society of Würzburg, at the same time lauding his efforts on behalf of German scholarship, "for many a scientific post was able to resist the financial pressure of those days solely thanks to his organization of German-American aid." Similarly, Brockhaus' encyclopaedia celebrated his "grosszügige Organisation der Unterstützung der deutschen Wissenschaft."⁵

Maintaining singular mental alertness, Boas remained at Columbia long beyond the usual length of service. He became "emeritus in residence" in 1936, emeritus in 1938, but retained his old office at the University. As a septuagenarian he continued to loom large at international congresses, still made trips to Europe, and continued to inspire students and visitors from foreign parts. His declining years were fraught with sadness. He lost his younger son and his wife in automobile accidents, and his second daughter through an insidious disease. Such solace as was possible he found in unremitting scientific work.

The rise of Hitler stirred him to the depths of his soul. That the country whose cultural heritage he gloried in, the country on whose behalf he had suffered abuse and ostracism in the first World War, should flout the principles dear to him was an unbearable thought. Besides, being of Jewish extraction, he had relatives in Germany whose very existence was threatened by the *Umbruch*. He reacted once more in character, writing an open letter to President Hindenburg, denouncing the tenets of Nazism in the daily press or in popular magazines; dragging himself when already enfeebled by old age and an encroaching heart disease to the platform at public gatherings in order to inveigh against Hitlerian excesses.

His campaign against racism naturally brought him a wider following than the monographs on the Kwakiutl or even the academic treatment of race in *The Mind of Primitive Man* (1911, 1938). He became the spokesman not only of disinterested humanitarians, but also of Leftists and Communists. Communists

⁵ Vol. III (1929), 66.

are not universally popular, and even in quarters averse to Nazi philosophy the association with them sufficed to make Boas a suspect fellow-traveler. The facts seem to be as follows. Boas had a live social sense that automatically made him favor the underdog, so that he was unquestionably a liberal rather than a conservative in his general outlook. On the other hand, he loathed regimentation, whether by a college president, a party machine, or an unenlightened public opinion. When Lily Braun, the renegade daughter of a Prussian general, published her memoirs in 1908, Boas read them and was repelled by their picture of Social Democratic party tyranny. In possibly my very last conversation with him, a year or so before his death, he broached the subject of the Bolsheviks, summarizing his position in these words: "The Communists have done many very good things, they have also done many very bad things." Assuredly this was not the voice of blind partisanship. As for Marxist doctrines, he had all his life recoiled from closed systems, hence could not accept a philosophy of economic determinism or any other dogmatic scheme. On the other hand, no one was less likely than he to avoid contacts simply because they might arouse general disapproval. "Fellow-traveler," "pink," and "red" were to him meaningless catchwords.

On December 21, 1942, Boas was lunching with Professor Paul Rivet (Paris) at the Columbia Faculty Club in the company of several colleagues. The guest of honor has graphically recorded the experience.⁶ Boas had just voiced his contempt for racism, when the fatal stroke occurred: "Sans un cri, sans une plainte, nous le vîmes se renverser en arrière; quelques râles, un grand cerveau avait cessé de penser."

Boas's services to anthropology were so great and manifold that occasionally enthusiastic disciples unfamiliar with history talked and wrote as though his predecessors and contemporaries were negligible. One obituary article declared: "He found anthropology a collection of wild guesses and a happy hunting ground for the romantic lover of primitive things; he left it a discipline in which theories could be tested and in which he had delimited possibilities from impossibilities." This is to parade Boas as a mythological

⁶ Paul Rivet, "Franz Boas," *Renaissance*, I (1943), 313 f.

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culture-hero creating something out of nothing. The conception would have been intolerable to Boas, who fully esteemed what had been done by E. B. Tylor, Lewis H. Morgan, Eduard Hahn, Karl von den Steinen, and others. Indeed, he was especially appreciative of men who had achieved what he himself never attempted—an intimate, yet authentic, picture of aboriginal life. I have hardly ever heard him speak with such veritable enthusiasm as when lauding Bogoras' account of the Chukchi, Rasmussen's of the Eskimo, Turi's of the Lapps.

In the following paragraphs, then, I shall try to sketch Boas's achievement in perspective and without unfairness to others.

To begin with an obvious fact, he approached the study of man from every angle: as Rivet puts it, "*son oeuvre embrasse le problème humain dans son entier . . . Tout ce qui concerne l'homme sollicite sa curiosité . . .*" What is more, from the start he saw the need of acquiring in each branch of the science the highest degree of technical equipment. The physical anthropologist must use the tools of biometrics; the linguist must become a phonetician and an analyst along the lines of Indo-European philology; the ethnographer must envisage the subtler as well as the more obvious phases of social life—folk-literature, music, the subjective attitudes of primitive man no less than artifacts or social structure. Nothing is more remarkable than the systematic way in which Boas, trained in quite different fields, acquired the techniques requisite for the highest type of work in the several subdivisions of anthropology. Even in archaeology, which he treated with comparative neglect, his work has been declared to show "a perfect appreciation of the problems and the best archaeological techniques."

Further, this many-sided virtuosity was justified by the solidity of his results. Everywhere he saw new problems and devised new methods of attack. Even his archaeological contributions, Mason assures us, "all have been substantiated by later and more detailed work. They have formed the basis for all later research in this region." What is more, they preceded by several years the stratigraphic approach that rightly shed luster on Kidder's and Nelson's work in the Pueblo area. Again, in linguistics, Boas was,

if not the first, yet the most persistent "to analyze exotic material without forcing it into the strait-jacket of the familiar" (Emeneau). As a physical anthropologist he deprecated sheer taxonomy; defined race on a profounder basis; demonstrated the (*nota bene*, limited) plasticity of the human organism; studied the phenomena of growth on a major scale; and was one of the earliest investigators to note segregation in hybrid human groups. His ethnological contributions were so varied that two must suffice for purposes of illustration. He was the first to inquire into the aboriginal artist's subjective attitude toward his tasks; and, paralleling the work of Homeric scholars, he correlated the social life depicted in a people's folk-literature with their observed culture. In theory he may be described as an epistemologist rather than a metaphysician: he suspected traditional labels and catchwords, inquired into their empirical foundation, and often arrived at a new and illuminating re-classification of data.

Tastes differ in science, as in everything else. Hence Boas's achievement was bound to disappoint certain minds. Keenly aware of the gaps in our knowledge, he refused to fill them with plausible speculations resulting in a spuriously complete picture of the whole field. He proclaimed no all-bracing "laws" and, except for his views on race, voiced no simple message that might appeal to large masses. In point of form he lacked the polished diction of a Frazer or the sprightly humor of his friend, Karl von den Steinen. Nor did he complete a single large-scale portrait of a tribal culture, not even of his beloved Kwakiutl.

Similarly, his teaching was not designed for everyone's palate. The most effective trainer of anthropological investigators was not an ideal pedagogue. He was, indeed, uncanny in his capacity to harness a student's skills for the advancement of science, but he did not trouble to ferret out a learner's needs at a particular stage of progress. Novices were not pampered with milk for babes. Fearful lest they turn dilettanti, he imposed on virtually every newcomer in my day his course on statistical theory (usually audited by professors from other departments) and another on American Indian languages. His ethnographic lectures rarely, if ever, systematically surveyed the area announced, but discussed the problems that engaged his attention. Other men's views he often

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treated in a way likely to mislead the immature, for by concentrating on controversial issues he sometimes conveyed the impression of total condemnation when there was merely partial dissent. One might easily carry away the idea that he had a low opinion of Tylor or Ratzel, as was certainly not the case. His critique of environmentalism, for instance, was urged so forcibly that for years I failed to grasp how carefully he took cognizance of geographical factors. As to the skepticism he instilled by precept and example, he himself was at times smitten with qualms, wondering whether he was inhibiting the free play of the imagination, which, contrary to appearances, he rated very high. One student summarized his total reaction after a seminar of Boas's as follows: "All books are bad, articles *may* be good," the suppressed implication being that even they seldom were.

Yet he valued high-class work even when done by men of utterly different personality. Of Bogoras and Rasmussen I have already spoken. He keenly appreciated Francis Galton, William James, William Morton Wheeler, Karl von den Steinen. Of his Columbia colleagues I think he rated E. B. Wilson highest. "*He is a first-rate man,*" he once said to me. Thomas Hunt Morgan he accepted as "very good," but with qualifications. Among Washington scientists, Karl Grove Gilbert enjoyed his esteem. Contrary to opinions occasionally heard, his scientific judgment was little warped by personal animosity. There was not much love lost between him and certain Washingtonian colleagues; but he described one of his bitterest enemies to me as a man of great native ability and gave another full credit for founding a technical journal.

To revert to his teaching, my novitiate probably came at the worst possible period for establishing rapport, for it was the time of his feud with the director of the American Museum. Boas seemed perpetually busy and preoccupied. I actually dreaded meeting him on the way to classes in Schermerhorn Hall. Utter silence would follow a curt "Good morning" till I found the situation intolerable. "Have you read Kollmann's article on Pygmies in the last issue of *Globus*?" I once asked him on one of these embarrassing occasions. He answered, "No"; I offered a few remarks on the subject; then we again walked on in silence. Having a con-

ference with him was something of an event for A. B. Lewis, A. A. Goldenweiser, Paul Radin, and myself; Speck and Sapir, with their philological background, enjoyed, I think, a rather easier *entrée*. This also had held true in Kroeber's and William Jones's time, and as a visitor in later periods I was able to watch his free and easy relations with subsequent generations of his disciples. Accordingly, I cannot but ascribe his earlier reserve to the tribulations of the era.

Systematic information, as indicated, he did not vouchsafe in ethnological courses, *that* the student was supposed somehow to get for himself. Yet it was not an easy task at a time when the good books had grown antiquated, so that trustworthy knowledge was obtainable only by wading through tomes of unilluminating descriptive detail. However, Boas was singularly unexacting in regard to a student's factual information. Probably there is not nowadays a single undergraduate major in any of our large anthropological departments who does not control a wider range of data than I did when Boas deemed me fit for the doctorate. It was enough that I had worked in the field, gained a theoretical conception there, and thrashed out the issue in a formal paper. On the other hand, he came very near holding up A. B. Lewis, whose knowledge was incomparably superior to mine, but whose dissertation discussed nothing of theoretical significance. Berthold Laufer, who liked it, observed querulously to me, "Boas always wants a thesis to have a point!"

Why did we reverence so indifferent a pedagogue as a great teacher? For the same reason, no doubt, that in later years mature men and women—Elsie Clews Parsons, Pliny Earle Goddard, and George A. Dorsey, for example—hailed him as their leader. Yet Goddard had come to New York full of skepticism about Boas; and Dorsey had been at swords' points with him in the American Anthropological Association. The explanation is simple. Here was a scientist primarily interested in science—not in the *organization* of research, not in the personalities of colleagues, not in a display of his cleverness, but in the problems that sprang from his data, in the quest of the truth. He seemed to personify the very spirit of science, and with his high seriousness—unsurpassed by any investigator I have known in any sphere—he communicated some-

thing of that spirit to others. Therein lies his greatness as a teacher.

Constituted as he was, he could not avoid misunderstandings either as to his views or his character. Even scientific guilds live by slogans and balk at finer distinctions. Boas threw out a hint how totemism might have evolved in British Columbia and was forthwith credited with a universal theory of the phenomenon. Pointing to the positive achievements of colored races, he rejected the arguments of racists, hence was either hailed or denounced as a dogmatic equalitarian. Yet he clearly formulated in both editions of his most popular book a rather different position: "It may be well to state here once more with some emphasis that it would be erroneous to claim as proved that there are no differences in the mental make-up of the Negro race taken as a whole and of any other race taken as a whole, and that their activities should run in exactly the same lines."⁷ Again, his championship of a strictly limited plasticity was misinterpreted as a denial of heredity. Some forty years ago, at a joint meeting of anthropologists and psychologists, even his friend James McKeen Cattell contrasted Boas's environmentalism and Thorndike's emphasis on heredity. Boas was at once on his feet, protesting that he, too, attached very great importance to heredity.

Boas's aversion to systems and sweeping generalizations lent color to the charge that he was absorbed in detail—content, like Browning's Grammarian, with settling *Hoti's* business and giving the doctrine of the enclitic *De*. For, sharing Bastian's and Had-don's eagerness to rescue rapidly vanishing data, he did devote enormous energy to securing and making accessible raw material. It is easy to go through a thousand pages of his monographs without encountering a line of interpretation. But that was only one side of him and, of course, the least interesting. The faithful recorder was, above all, a thinker. I remember his suddenly electrifying a seminar with the statement that he was concerned with detail only as a way to understanding human mentality. On another occasion he quoted von den Steinen's saying that we must look at primitive man without the spectacles of our civilization; Boas amended it to read that we must look at *ourselves* without spectacles. He was ever aware of the preconceptions with which,

⁷ *The Mind of Primitive Man* (New York, 1938), p. 270.

as Virchow once put it, we are all "crammed full from infancy on." He once told me how hard he had had to struggle to overcome early rationalistic influences; and the burden of all his ethnological teaching, paralleling his linguistic position, was that every philosophy and form of behavior must be apprehended from the insider's point of view.

As explained, he was not a doctrinaire on the subject of race. It is worth adding that he was not a sentimentalist either. He befriended Indians, but unless (like Jones) they had qualified academically he did not welcome them to his classes. Similarly, he was strongly suspicious of any prospective disciples who were goaded by a romantic interest in the noble Red Man rather than by the urge to advance knowledge.

Notwithstanding his neglect of customary canons of presentation, Boas was far from lacking in aesthetic appreciation. Characteristically his abiding ethnological interests were primitive art and oral literature. A devotee of music, he played the piano very well for an amateur. Above all other composers he revered Beethoven. "To think that it was possible for such greatness to exist!" I have heard him say. Chopin, on the other hand, repelled him as morbid. In literature he naturally admired Goethe. Some years ago I asked him whether he still occasionally read him. "Of course," was the instant reply. Sheer wit or glamor had no appeal. George Bernard Shaw palled on him. I cited Heine as a parallel, only to have my defense parried with "Well, doesn't *he* tire you?" He was also very critical of Max Reinhart's staging, but somewhat grudgingly admitted the effectiveness of his production of Büchner's *Danton*.

That Boas made enemies is an empirical fact. Its explanation lies partly in circumstances, partly in his personality. For any adult immigrant to adapt himself fully to the folkways of his adoptive country requires a prodigy of flexibility and tact, especially in the peculiarly exacting atmosphere of an Anglo-Saxon society. Boas often appeared ruthless when from his own point of view he was merely candid. He could certainly be very blunt when matters of principle were involved, but then the personalities who make history have rarely been marked by a dainty concern for the sensibilities of queasy souls. On the other hand, no one could be more

understanding and kind in basically human situations. As the factotum of the department at the American Museum once confided to me, Dr. Boas had been a strict master, but a generous contributor to any employees' fund. Once, too, Boas disposed of some of his insurance in order to aid a former student in an alleged crisis; he entered with fullest sympathy into the feelings of a young lover or newlywed; and no one could write tenderer notes of condolence on the occasion of a bereavement.

But where no fundamental human factor was involved, empathy was too readily blotted out by contrary emotional urges. In a seminar he once referred to a map in the Swedish journal *Ymer*. "What language is the article in?" asked a student of great erudition, but little initiative. "The map can be understood independently of the language," Boas snarled back. Estrangements from one-time students were in part merely the familiar phenomenon of filial revolt, but in part they resulted from Boas's taking a rational point of view that clashed with the disciple's emotional urges. He was wont to survey the chessboard of anthropological jobs and figure out how science could be best served, then he would try to move anthropologists about like the pawns in a game. His judgment was usually right, but some men and women resented the impersonality of his strategy. One case may be cited as typical. He had secured for an outstanding student an excellent position with superb research opportunities, but in a city without a university and meager in cultural facilities. After a few years the incumbent grew restive, felt marooned, and eagerly accepted a metropolitan appointment—much to Boas's surprise and disgust. The master found it difficult to understand that extra-scientific motives should have tipped the scales.

Deficiency in empathy was naturally intensified when principles seemed at stake. It was not easy for him to do justice to an ethically uncongenial attitude. In 1919 he excoriated four anthropologists who had mingled intelligence work with research in Latin America during the War. It did not occur to him that, from their point of view, they had been merely discharging a patriotic duty. In other cases he was unwilling to make allowances for human frailty. In 1933 he could not understand the conduct of Germans who welcomed Hitler even when they repudiated his racist pro-

gramme. The point is not that he disapproved, but that he seemed unable to project himself into the mental state of men who were at once kindly and fervidly patriotic, who were in other words caught in a fearful conflict of humanitarian and nationalistic loyalties. He forgot that one cannot expect every man to be a hero.

Such absorption in his own ideals was, of course, from another angle part and parcel of his greatness. In trying to boil down my admiration for him into a few words, I find that I have been forestalled by Wundt's eulogy on an otherwise very different personality, Gustav Theodor Fechner: "absolute lack of bias [due to tradition] and intrepidity." The mere fact that a view was universally accepted and supported by eminent authority was precisely a ground for skepticism in both Fechner's and Boas's case. Observations Boas had not himself made he was likely to challenge or at least to mistrust. Meeting him in Berkeley in 1914, I dropped the innocent remark that there were many tall women about town. Without directly denying the statement, Boas pooh-poohed its significance: "In a population whose males average 175 cm. [he was never at ease with feet and inches] you must expect to find some tall women." A few days later he remarked, "Aren't there many tall women in Berkeley?" He had by that time observed for himself.

Qualities and men are rightly prized for their rarity. Boas had unusual intellectual powers and extraordinary devotion to science, yet if I were to single out his unequivocal claim to greatness I should rather stress the qualities he shared with Fechner, for it was these that mark him as a figure to be aligned with those who have made human history. The correctness of his attitudes seems quite immaterial; what counts is his remaining true to his vision, with total disregard of whether the mob stigmatized him as "pro-German" at one stage or as "Communist" at another. To have known such a man in the flesh is what I esteem above any of his specific teachings as, once more to quote Wundt, "an inalienable gain of my life" (*einen unverlierbaren Gewinn meines Lebens*).

[The Selected Bibliography of Franz Boas appended to this paper has been omitted.]

Some Problems of Geographical Distribution

FOR AN ETHNOLOGIST THERE IS NO MORE FASCINATING PART of *The Origin of Species* than the discussion of geographical distribution. Why, asks Darwin, do quite diverse faunas and floras occur in strikingly similar physical conditions? Why do Australia and South America share not a single indigenous mammal with Europe, seeing that European species deliberately introduced into those continents have been thoroughly naturalized there? What is the meaning of intermittent occurrences? Why the affinity of the Galapagos fauna with that of continental South America? Why are there bats, but no frogs on oceanic islands? The investigator of human societies grapples with comparable problems; and he attacks them—*mutatis mutandis*—with a parallel logic.

Naturally distinctive considerations enter because of the differentiae of *Homo sapiens*. Whereas Alpine plants related to arctic forms must have literally migrated to their habitat in a period of climatic change, human groups *can* spread their arts and customs without travel. Given speech as a medium of communication or

Südseestudien (Basel: Museum für Völkerkunde und Schweizerischen Museum für Volkskunde, 1951), pp. 11–26.

even intertribal contact that permits mimicry, a feature of social life can pass from one population to another over a vast territory. Again, though the same physical environment does not generate identical species, the same geographical, demographic, and social conditions may very well produce the same results in distant areas, not indeed duplicating whole cultures, but identical adaptations or even series of parallel features. Thus, tribes living in flood-menaced districts independently hit on the dodge of raising their huts on piles. People among whom nubile women outnumber men tend toward polygyny. A group that subjects another develops a stratified society, with the conquerors as the overlords.

In ethnological practice distributional enigmas constantly crop up in two main forms. Some phenomena are spread continuously over a sizable area, others turn up in two or more distant regions. The former category at first blush present no serious problem. To take a simple example, Swiss peasants play two mutually exclusive games that fall under the general head of bowling—skittles and *boccia*. The latter, known on French soil as *jeu aux boules*, is narrowly restricted within the Swiss republic to the Ticino and the vicinity of Geneva, but extends through upper Italy and the south of France. Wherever it occurs in Alemannic territory, it has been demonstrably carried by migrant Italian or Ticinese laborers. In short, we are dealing with a diversion diffused by speakers of the two Romance tongues.¹

However, even this seemingly transparent set of data brings us face to face with an abiding puzzle. There are skittles and skittles: as one soon learns in driving through western Swiss villages, there are not only "German" alleys, so labeled, but also "French" ones. Now, according to the authorities on the subject, the French form of the game is receding, being supplanted by the German even in the west. Why? one wonders. What would induce any group to give up a familiar form of amusement in favor of another of the same order? But more generally, what makes human societies take up certain novelties of which they take cognizance and reject others? Alemannic skittles thus soar to the plane of Tennyson's "flower in the crannied wall": if we could really ex-

¹ Richard Weiss, *Volkskunde der Schweiz* (Erlenbach-Zürich, 1946), p. 193, map.

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plain their triumphant advance on broad principles, we might perhaps not get nearer to God, but we should assuredly know a good deal more than we do about human behavior.

It is manifestly impossible to treat geographical distribution adequately within reasonable limits of time and space. I shall therefore henceforth confine myself to "intermittent distribution." Here either of two antithetical explanations is a priori possible. On the one hand, the distribution may once have been continuous. Man can spread a craft or belief through successive contacts with alien groups until it covers a considerable territory; or by controlling means of transportation he may himself carry it to a distant region, as in the case of Polynesian seafarers. Yet specific conditions may cause the item to drop out, so that occurrence becomes sporadic. Thus, in the temperate climate of New Zealand the tropical species cultivated elsewhere in the South Seas no longer thrive; so in Africa, tsetse-haunted districts cannot maintain cattle. On the other hand, a trait may conceivably arise independently. It may spring from adaptation to like circumstances, as when swamp-dwellers put their huts on piles; and even unlike causes may yield convergent results, as when people inhabiting a dry region also elevate their dwellings as a protection from sudden attacks.

Let us concentrate on the major dichotomy: historical connection versus independent development. When is it proper to infer a genetic connection between two occurrences in separate areas, when can we safely predicate independent development? In discussing the logic of the case, Edward B. Tylor, the leader of British anthropology in the nineteenth century, laid down certain postulates that have never been bettered. Everything, he argued, hinges on the complexity of the trait discussed. "If Englishmen landing on a remote island were accosted by natives in their own language, the notion that English had been developed here as well as in England would be treated as a jest." At the opposite pole, Tylor would have admitted, are such simple, transparent adaptations as pile-dwellings in swampy country or the rise of plural marriage where there is a surplus of either sex.

The overwhelming majority of discontinuously occurring traits are intermediate in point of complexity, hence, as in most interpretations, an element of subjectivity enters. To quote Tylor once

more, there is his classical instance of lot games, in which two-faced objects are thrown, yielding the equivalents of our "head" and "tail." Described as *patolli* by early Spanish chroniclers in Mexico, known as *pachisi* in India, *nyut* in Korea, *tab* in Egypt, the game is also widely current north of the Rio Grande. Is it conceivable that this form of amusement arose independently at least twice, in an Old and a New World center? Tylor thus summarizes the items of correspondence: There are not true dice, but lots; the players stake property on the outcome; several lots are combined in scoring, account being taken of the lesser and the greater probability of throws; the results are transferred to a counting-board; the rules for moving and taking are similar. This concatenation of details puts the parallels "far outside any probability on which reasonable men could count," hence the evidence suggests to Tylor pre-Columbian "communication across the Pacific from Eastern Asia."²

By what route the trait was spread we do not know; but it is at least equally obscure by what psychological channels two unrelated populations would develop the same complex game. The study of distribution thus opens up new vistas of historical contact. What we insist upon, on methodological grounds, is that an argument for diffusion must rest on the comparison of sharply defined features. Thus, in the study of mythology and folklore the mere belief in, say, a man in the moon is of no significance. At the opposite pole, as conclusive as the recurrence of English speech on some remote shore, stand the following parallels.

An Albanian droll runs as follows:

"One day several friends came to Nastradin in order to borrow his donkey for one day. 'I have no donkey,' said Nastradin. At the very moment the donkey in his stall began to bray. The men said: 'Why, there he is, braying!' 'I am surprised,' said Nastradin, 'that you believe the donkey rather than me.'"

Now, in Greater Boston our one-time student, Dr. Dorothy Demetracopoulou Lee, collected the following:

² E. B. Tylor, "On the Game of Patolli in Ancient Mexico and Its Probably Asiatic Origin," *RAI, Journal*, VIII (1879), 116-129; *idem*, "On American Lot-Games as Evidence of Asiatic Intercourse before the Time of Columbus," *Internationales Archiv für Ethnographie*, IX (1896), 55-67.

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"One morning someone came to Stradi Hodjas to borrow his donkey; and he did not want to lend him. He says: 'He isn't here.' At that moment, the donkey brayed. Says the man: 'There he is. You lied to me.' Says Stradi Hodjas: 'Will you believe *me* or my donkey?'"³

Needless to say, Mrs. Lee did not obtain the story from the Cabots of Back Bay; she heard it from Greek immigrants in their native tongue. Greece and Albania are neighboring countries and in the past folkways as well as folk-tales have again and again crossed the ethnic frontiers of Southeast European countries. There is thus no difficulty whatsoever in understanding how Albanians in Europe and Greeks of Boston would share a quaint jocular anecdote. Indeed, we know from other sources that neither people has originated the story, that both have borrowed from the *Turkish* cycle attached to a quaint character called Nasreddin or Nastradi and enthusiastically taken over as the most acceptable correlate of Turkish domination. No one, of course, could deny the genetic unity of the two versions: It is utterly inconceivable that two groups would separately invent the identical episode with its climax and even give to the central character virtually the same name to boot. The case for historical connection is thus perfect.

As a matter of fact, such wealth of corroboratory evidence is hardly necessary to carry conviction. A noodle story known from northern Europe and from the Causasus offers an equally unchallengeable instance through the sheer oddity of its central motif.

The Lapps thus describe an adventure experienced by a trio of Savolacen, a legendary race of nincompoops:

Three Savolacens had gone to a bear's cave, and one of them went to bind the bear. He set about it so awkwardly that the bear grew angry and bit off his head. The two others outside got tired of waiting and pulled the third man out again. When they saw that his head was missing, they asked each other whether he had really had a head at all when he went inside. After pondering the question for a while, one of them exclaimed: "Yes, he must have had a head, I recall his having a beard."

³ Maximilian Lambertz, *Albanische Märchen*, Schriften der Balkankommission, Linguistische Abteilung, Akademie der Wissenschaften (Wien, 1922), pp. 54 f.; Dorothy Lee, "Greek Tales of Nastradi Hodjas," *Folk-Lore*, LVII (1946), 191.

In the Caucasus mountains the natives of Auch play the part of simpletons, whose mentality is mirrored in the following episode:

Three men from Auch were once traveling and somewhere found a cave. They guessed that presumably a fox was inside, and one of them crawled in. But within the cave dwelt a bear, who with one blow of his paw tore off the intruder's head. When the man gave no sign of life whatsoever, the two others pulled him out and found his head lacking, but neither could recollect whether he had ever had one. So one of them ran into the village and asked the dead man's wife whether her husband had really once had a head. "I really cannot recall for sure," she answered, "but this much I know as a certainty, that every year I made a new cap for him." ⁴

Obviously the Lappish and the Caucasian variants stem from a single original, whether or not the path of diffusion be discernible. But in this instance the geographical gap is, at least in a measure, spanned. Our story is familiar to Finns as well as to Lapps; and Finnic populations reside as enclaves in eastern Russia. The Syrian extend from the 58th degree to the Arctic Ocean, and from the Urals westward to the Pinega, an affluent of the Northern Dvina, which brings them within striking distance of the Western branch of the stock. On the Kama, a tributary of the Volga, dwell the Votyak, who belong to the same family. A third Finno-Ugrian people, the Cheremiss, spread over districts southwest of Kazan; and the Mordvin occur on both sides of the Volga, extending south of Samara. This brings them, if not near, yet appreciably nearer to the Caucasus.

To take another example from folk-literature, on my first field trip, in 1906, I heard the following myth among the Shoshone of central Idaho:

In the beginning Wolf wanted to make everything easy and pleasant for the Indians, Coyote tried to make them work hard as they must do today. Wolf had all the game shut up so that people could easily take what they wanted; Coyote released the game so that people had to go hunting. Wolf said: "The Indians shall not

⁴ Johan Turi, *Das Buch des Lappen* (Frankfurt am Main, 1912), p. 259; James Lloyd Bowman and Margery Bianco, *Tales from a Finnish Tupa* (Chicago, 1936), p. 227; Adolf Dirr, *Kaukasische Märchen* (Jena, 1922), pp. 279 f.

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die." "Why should they not die?" asked Coyote. "They must die." An Indian was lying sick in his tent. The medicine-man was treating him, but he died.⁵

This tradition, also common in central California, has for its essential feature a dispute about the future of mankind between Wolf, the elder brother, and his junior, Coyote. Wolf wishes people to live at ease, but Coyote overrides the suggestion and in a corresponding argument introduces death into the world. Now, at the extreme tip of the Western Hemisphere the southernmost people of the globe, the Yahgan, describe precisely such a fraternal controversy. The elder of the two Yoalox brothers is eager to save the Indians trouble, but the younger doggedly opposes him. The narrative is far too long to be quoted *in extenso*, but snatches of the dialogue convey its flavor. After the invention of fire-making the benevolent brother declares: "Let it burn forever, so that it shall never go out. Once lit, let it maintain itself always, so that men may use it without trouble . . ." The marplot at once counters with: "It is much better that men should exert themselves . . . Men shall work!" Again, the senior brother proposes that henceforth birds shall be killed by a mere stare, but his interlocutor spurns the idea: "It is much better for men to manufacture weapons and implements to take along on a hunting trip. Let them exert themselves and use cunning, sneaking up to birds for a surprise attack, so as to kill and seize them. Thus it shall be, everyone shall have to work and take pains." The younger brother's insistence similarly establishes death.⁶

Of course, it is not dualism by itself that is significant here. In aboriginal lore, one brother or comrade often figures as a foil for the other, as when a Brazilian Indian myth sets off the power and cleverness of the Sun in contrast to Moon's stupidity. But our tale falls into a different category altogether. The opponents do not play tricks on each other nor quarrel about personal issues. They are at odds over the arrangement of life for mankind in the future; they debate and decide the question of human mortality; and on

⁵ R. H. Lowie, *The Northern Shoshone*, AMNH, Anthropological Papers, II (1909), 239.

⁶ Martin Gusinde, *Die Yamana* (Mödling bei Wien, 1937), pp. 1159-1183, 1240 ff.

each point the younger carries the day. These are the items which Basin and Californian Indians share with the Tierra del Fuegians, and a scrutiny of the literature on the immense intervening territory yields nothing comparable.

The argument for a genetic connection thus seems cogent. From the parallel cited it appears that at some, presumably distant, period in the past the ancestors of the Fuegians lived much farther north, more or less in proximity to the ancestors of recent Basin or Californian Indians; in migrating southward they brought with them not only, as everyone grants, simple crafts, such as stone-flaking, but also some of the ancient aboriginal folklore.

Sometimes the puzzles of intermittent distribution can be solved with auxiliary information from another discipline. In 1916 Sapir pointed out that the word for tobacco, *op* (up), was virtually identical in the languages of the Diegueño of Southern California, the Shasta in northern California, and the Takelma of southwestern Oregon, three tribes of quite distinct linguistic affinities. As Sapir inferred, the evidence indicates the gradual diffusion of tobacco over western North America. In this case, as often in the Old World (though more rarely in the New), a cultural phenomenon has spread, together with its linguistic label.

Hundreds of miles to the northeast of these Pacific coast tribes, the Crow Indians, a member of the Siouan family living in southeastern Montana, had long been known to raise a form of tobacco for purely ceremonial purposes. The term for this plant in Crow and in Hidatsa, the only closely related Siouan tongue, is *op*, differing widely from the semantic counterparts in more remotely connected Siouan languages, where we find such stems as *nini*, *tcani*, *indukabi*. Why should one branch of the Siouan stock resemble in this respect three wholly unrelated stocks while sharply differing from fellow-Siouans?

In 1910 I was permitted to photograph tobacco plants growing in a Crow garden and purchased a complete specimen. From the print and from the plant itself the late Professor W. A. Setchell identified the species as *Nicotiana multivalvis*. Unknown apart from cultivation, he explained, it could be produced by selection from the ordinary wild tobacco (*Bigelovii*) of northern California. It is furthermore reported to have been formerly grown by a tribe

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near the falls of the Columbia River. A combination of ethnographic, linguistic, and botanical evidence thus establishes a connection, otherwise unattested, between the natives of eastern Montana and those of southernmost California.⁷

So far I have discussed games, folk-tales, and a ceremonial plant; the distribution of practical devices is equally suggestive. We may proceed from the postulate that a clever contrivance will not be lightly dropped. Indeed, at times it will persist when on rational grounds it should be superannuated by a new and superior way of accomplishing the same end. On the other hand, circumstances do *confront men with the alternative of either dropping a useful invention or modifying it.*

In the eighteenth century Du Halde thus described a Chinese method of hunting water-fowl:

They put their heads into large hollowed and dried gourds with holes permitting them to see and breathe. Then they go into the water or rather swim therein, not allowing anything to be seen but the head sticking in the gourd. The ducks, being accustomed to such floating gourds, with which they are wont to play in the water, approach them without fear; but the hunter at once seizes them by their feet, and lest they cry out he at once twists their necks and hangs them to his belt.

Compare this with Beals' report of Indian practice in northern Mexico:

A number of calabashes were allowed to float on the water frequented by the waterfowl for a number of days until they became accustomed to them. Then the hunter donned a similar calabash perforated to admit the head and with holes to permit seeing. With only the calabash above water, he moved along among the waterfowl, seizing them by the legs and drawing them under, wringing their necks and fastening them to a belt.⁸

⁷ Edward Sapir, *Time Perspective in Aboriginal American Culture*, Canada Geological Survey, Memoir 90 (Ottawa, 1916), p. 70; R. H. Lowie, *The Tobacco Society of the Crow Indians*, AMNH, Anthropological Papers, XXI (1919), 111, 198 f.; Melvin R. Gilmore, "Some Comments on Aboriginal Tobacco," *American Anthropologist*, XXIV (1922), 480 f.; William A. Setchell, "Aboriginal Tobaccos," *American Anthropologist*, XXIII (1921), 408 ff.

⁸ Erland Nordenskiöld, *Comparative Ethnographical Studies* (Göteborg, 1924 and 1931), III, 65, IX, 43; Ralph Beals, *The Comparative Ethnology of Northern Mexico before 1750*, Univ. Calif. Ibero-Americana: 2 (1932), p. 104.

It is difficult to imagine an independent origin for this ingenious and sophisticated technique, which occurs also in the Antilles, in Chiriqui and Mojos. In modern times, no doubt, as the records of our patent offices prove, even complicated inventions have been duplicated. But the case is not comparable. In global perspective modern scientists, whether working in Japan, Russia, or the United States, belong to a single culture, that of industrial civilization. In other words, they have a common background of scientific knowledge, a common impetus to advance learning and its applications. Nowhere else do we find groups of trained specialists in control of the same techniques and deliberately striving for the solution of similar problems. That a Chinese and a Mexican Indian with their utterly different cultural traditions should each excogitate so elaborate a hunting trick is assuredly improbable.

To turn to another matter, cooking is one of the most ancient applications of fire, but not all its forms are equally old. Australians, Fuegians, Ge, even Polynesians, broiled victuals on a spit, roasted them in ashes, or baked them in earth-ovens, but could not boil them as we do ours. Bamboo vessels are of course possible only where the woody grass is available; metal is beyond the reach of a Stone Age economy; earthenware, though widespread, is by no means universal and was unknown to the peoples mentioned. However, a fair number of potless tribes did boil food after a fashion, by the crude technique called "stone-boiling"! the cook put his food and water into a container, heated rocks till they were red-hot, and then dropped them into his vessel. The natives of British Columbia, expert carpenters, thus used wooden boxes; northern Californians, water-tight baskets; Plains Indians, a pit lined with a hide; and the Chono of southern Chile, bark buckets. Several peoples, like the Polynesians, who rely on other forms of food-preparation, fall back on stone-boiling as a subsidiary process. Samoans, e.g., thus cook liquids in a wooden bowl.

The most curious instances, however, are those of peoples who own vessels suitable for boiling, use them for that purpose, but at the same time retain the messier procedure of stone-boiling. One case, that of the Kamchadal in eastern Siberia, has been

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plausibly explained by Jochelson; he suggests that they boiled in clay vessels when at home, but with hot rocks in less fragile containers of wood when traveling. This theory, however, no longer fits the examples from Europe. In 1732 Linnaeus saw Finns prepare a beverage called *lura* by dropping hot stones into it; quite recently, the Basques of Guipúzcoa heated milk by this method, and the Serbs are credited with the same practice. Yet Finns, Basques, and Serbs have been familiar with metal ware for centuries, if not for millennia. There seems to be no rational explanation for this phenomenon. We confront the capricious retention, in special circumstances, of an outmoded type of cookery, a psychologically revealing sample of what is technically known as a survival.⁹

A third eminently practical, if unsportsmanlike, practice is to drug fish in still waters by means of a narcotic plant. On the upper Rio Negro (Brazil) the natives fence off some quiet cove, pound up and rinse the fresh root of *Paullinia pinnata* in a canoe until the root is thoroughly frayed and a milky juice accumulates, whereupon the boat is turned upside down. The poison drugs the fish, which rise to the surface, exposing their abdomens, and are then easily taken. Details vary: in many districts of South America other plants supply the narcotic principle; sometimes one species of fish is immune to a particular poison that is effective with a different species, so that a single tribe may apply more than one plant poison; and the leaves, seeds, or fruit may take the place of the root. Geographical barriers may inhibit the method, inasmuch as narcotic species are wanting, as Norden-skiöld discovered south of Santa Cruz, Bolivia. The usage is highly developed in the tropical zone, including the Antilles, and spread as far north as Colombia, and to the Araucanians of Chile in the

⁹ E. B. Tylor, *Researches into the Early History of Mankind* (London, 1865), pp. 261–269; Te Rangi Hiroa, *Samoan Material Culture*, Bernice P. Bishop Museum, Bull. 75 (Honolulu, 1930), pp. 100–103; John M. Cooper, in J. H. Steward, ed., *Handbook of the South American Indians*, BAE, Bull. 143 (1946), I, 51; Curt Nimuendajú, *The Serente*, Publications of the Frederick Webb Hodge Anniversary Publication Fund (Los Angeles, 1942), VI, 34; *idem*, *The Apinayé*, The Catholic University of America, Anthropological Ser., No. 8 (Washington, 1939), p. 96; Waldemar Jochelson, *Archaeological Investigations in Kamchatka*, Carnegie Institution of Washington, Publ. 388 (1928), p. 68; Arthur Haberlandt, in Georg Buschan, *Illustrierte Völkerkunde* (Stuttgart, 1926), III, 292, 327 f.

south. As my colleague Dr. R. F. Heizer has shown in an as yet unpublished paper, fish-drugging is found also in North America. It occurs in California and in the interior of the southeastern United States, the northernmost limit probably being the Columbia River, with no evidence from Alaska and Canada.¹⁰

So far the dispersal of the trait presents no major difficulty. The Amazon-Orinoco area, where it is intensely developed, seems a likely center of diffusion; narcotic plants abound there, and numerous waterways offer marvelous opportunities for travel to the excellent boatmen of the Arawakan, Cariban, and Tupian families residing in the territory. With the Antilles as a stepping-stone, the custom could easily be imported into Florida, and though gaps remain in North America they are not of fatally major extent.

However, fish-drugging is by no means confined to the New World. Heizer finds in Aristotle's *Historia Animalium* a reference to *Verbascum* as a piscicide, and Pliny credits *Polyrhizon* with similar properties. According to Haberlandt, the technique was applied in medieval Germany, a *Fischbüchlein* first published in Nuremberg in 1735 listing 23 recipes for poisoning fish. He further finds evidence from modern Sweden, Portugal, Bohemia, Hungary, and the Balkans. Outside Europe the technique is known from various parts of Africa; from India, China, and Persia; from Malays, Polynesians, and Melanesians; from Australian Blackfellows, the primitive Andamanese, and the Vedda of Ceylon. In some cases, the same genus is applied for drugging fish in both hemispheres: *Tephrosia*, which figures among the Lobi of the Volta region, the South Nigerians, the Mangbettu of the Belgian Congo, and the North Queenslanders, serves the same purpose among the Makusi in British Guiana.¹¹

¹⁰ Theodor Koch-Grünberg, *Zwei Jahre bei den Indianern Nordwest-Brasiliens* (Stuttgart, 1921), pp. 256 f.; W. E. Roth, "An Introductory Study of the Arts, Crafts, and Customs of the Guiana Indians," BAE, 38th Ann. Rept. (1924), pp. 202-204; Nordenskiöld, *op. cit.*, II (1920), 40-45; *idem*, *Indianer und Weisse in Nordost-Bolivien* (Stuttgart, 1922), p. 17; Regina Flannery, *An Analysis of Coastal Algonquian Culture*, The Catholic University of America, Anthropological Ser., No. 7 (1939), p. 19.

¹¹ Arthur Haberlandt, *op. cit.*, pp. 318, 319; Henri Labouret, *Les Tribus du rameau Lobi*, Université de Paris, Travaux et Mémoires de l'Institut d'Ethnologie, XV (1931), 127; P. Amaury Talbot, *The Peoples of Southern Nigeria* (London,

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The question of inter-hemispherical borrowing thus arises once more. The universally admitted route of connection via Bering Strait certainly seems eliminated. Narcotic plants are extremely rare in northern Asia, Heizer points out, and evidence for fish-poisoning is wanting in that region. And, as we have seen, it is equally absent for the northern part of North America. There remains thus the transpacific route, which in some of my examples I have not ruled out on principle, but accepted as furnishing a reasonable explanation of the distributional facts. However, each case has to be considered on its merits. It is exceedingly difficult to imagine seafarers from, say, Polynesia landing on the coast of South America and attempting to introduce fish-poisons in oceanic waters. On the whole, Heizer's conclusion that the natives of the two hemispheres independently arrived at the notion of drugging fish seems acceptable.

In discussing the historical aspect of the problem, however, we must not lose sight of the at least equally important psychological questions that arise. In the first place, what is meant by the diffusion of "fish-drugging"? Primitive man does not deal with the Platonic idea of a process, but with a definite species of plant and a particular part of it. To confine ourselves to the South American scene, the knowledge, then, that a properly prepared root of *Paullinia pinnata*—not any portion of any member of the vegetable kingdom—ensures a good catch is the significant thing, and this recipe, if anything, will be taken over by alien visitors.

Nevertheless, the conditions of life preclude absolute fixity. Exhaustion of the soil from which partly agricultural tribes derive much of their sustenance, defeat by enemies, often even purely superstitious motives, have again and again caused native communities to migrate. Often they inevitably will reach a territory where the familiar narcotic no longer thrives. What follows? Either the process must be abandoned, as happened south of Santa Cruz, Bolivia. Or, the immigrants seek a substitute until they discover one. Now, Dr. Heizer informs me that South American natives use at least 180 species for fish-poisoning. Hence, they must have deliberately experimented by the painful method

1926), pp. 918 ff.; Georg Schweinfurth, *The Heart of Africa* (London, n.d.), II, 40; Roth, *op. cit.*, p. 204.

of trial and error until they learnt the distinctive repertoires of the several regions. The amount of prerequisite observation cannot easily be overestimated. Indians cannot know a priori that a fish species immune to the otherwise effective *Lonchocarpus densiflorus* will succumb to *Tephrosia toxicaria*. There is further proof of rational adaptation when the indigenes of northwestern Brazil cultivate *Clibadium Schomburgkii* for the definite purpose of killing fish with its leaves.

Nor is this all. If a poison will stupefy fish, why not try it on land animals? Australians thus actually come to drug emus as well. (It is, of course, unimportant for the transfer of learning on which animal the narcotic was first employed.)

What is more, Nordenskiöld has convincingly suggested that one of the most remarkable achievements of primitive man derives from fish-drugging. Among aboriginal American crops, the bitter manioc (*Manihot utilissima*) is second only to maize in importance. In post-Columbian times it was introduced into tropical Africa, and the late Dr. Carl Alsberg assured me that its economic significance in the world is potentially far greater than has yet been realized. The extraordinary thing about the plant is that its juice contains hydrocyanic acid, a deadly poison, as Schweinfurth discovered about seventy-five years ago, in the upper Nile country, where he jotted down the following observation:

"While we were here, one of the Bongo bearers died from the effects of eating manioc before it had been prepared and divested of its poisonous parts. For twenty-four hours before his death he had lain in a state of coma, and a strong emetic had been entirely without effect."

What, we ask, would stimulate savages in the first instance to transmute a fatally poisonous plant into their staple food? As Nordenskiöld points out, this result was probably achieved via a detour. Among many other species, the Indians had also used *Manihot utilissima* to drug fish. "By some chance it was subsequently discovered that the pulp which remained after the poison had been extracted could be eaten without ill effect." And when hard put to it, the natives systematically tried the purged material as a famine-food.¹²

¹² Nordenskiöld, *Comparative Ethnological Studies*, III, 36 f.; Georg Schweinfurth, *op. cit.*, I, 226.

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The topic of fish-drugging—an apparently minor feature of primitive economy—thus has significant ramifications and itself directly bears on such fundamental matters as aboriginal mentality and the psychology of invention.

Studies of distribution can do more than determine tribal connections or shed light on human mentality. At times they illuminate the causal or, to put it cautiously, the functional relations between descriptively separate phenomena. Let me illustrate by a classical example from the history of our science.

About ninety years ago a pioneer in sociological research, Lewis H. Morgan, made the surprising discovery that the nomenclature of kinship he had recorded in New York State among Seneca Iroquois was almost duplicated among the Tamil, a Dravidian people of southern India. The actual words for denoting various relations were naturally different, but the way of classifying such relatives as “father” or “mother” in either language was “nearly a literal transcript” of the other. Thus, in both cases the term most closely corresponding to our “father” embraced additional relatives, and the same ones. Morgan drew the far-reaching historical conclusion that there was a deep ethnic bond between natives of India and American Indians, who thus had not really been mislabeled by early discoverers. Indeed, he postulated not merely contact between the two, but a racial affinity.¹³

Had Morgan known the distribution of his phenomenon, he would have been spared this confusion of the biological and the cultural sphere. We have since learnt that American kinship terminologies are far from uniform; that while some resemble the Dravidian (as well as Australian, African, and Melanesian) systems, others correspond closely to our own. Had he recognized the fact, Morgan would certainly not have argued that the Iroquois were racially akin to the Dravidian, since the same line of reasoning would put the natives of Idaho and Nevada nearer to the European race than to Eastern American Indians.

However, Morgan was also concerned with interpreting the ultimate meaning of his kinship systems, which he derived from forms of marriage; and though his specific suggestions have proved largely erroneous, the grand principle that the terminologies are

¹³ Lewis H. Morgan, *Systems of Consanguinity and Affinity* (Washington, 1871), p. 387.

correlated with social custom remains basically true. That is to say, relatives are as a rule similarly designated because their social status is similar. To take the simplest instances, if an Iroquois and a Tamil have many "fathers" and "mothers," it is because many individuals play a parental role in social intercourse among these peoples.

It may be asked why we cannot explain these parallels as we did the mythological resemblances between California and Tierra del Fuego. The answer is that this would involve the most improbable assumptions. For example, the Hopi of northern Arizona (Shoshonean stock) have a kinship system that is much closer to the Tlingit system in Alaska and to that of the Crow Indians in Montana (Siouan stock) than it is to the system of their geographical neighbors and linguistic cognates. The similarity, then, must be interpreted on other lines than by past contact.

The sociological types of explanation rest on either the combination of two widespread forms of marriage known as the levirate and the sororate, or on social structure. The levirate is a woman's marriage with her *levir*, i.e., brother-in-law, specifically her deceased husband's brother; the sororate, a widower's marriage with his deceased wife's sister. These two extremely widespread and usually coexisting usages jointly account for the inclusive application of parental terms. For, where these forms of marriage are sanctioned and, as often happens, even obligatory, a child naturally views his paternal uncle as a prospective stepfather, as a potential spouse of the widowed mother. The paternal uncle may assume his dead brother's role, whereas the maternal uncle never could without violating the laws against incest, which are as a rule exceedingly strict. Correspondingly, a woman figures as her sister's matrimonial understudy. Thus, the combined action of levirate and sororate yields the traits of nomenclature to be explained, and furthermore, these two forms of marriage agree fairly well in distribution with the traits in question.

The alternative explanation rests on the social subdivisions known as "unilateral descent groups," of which clans are familiar examples. For certain purposes, such as property inheritance, many peoples recognize kinship on one side only, ignoring the

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other completely. Thus, among the Hopi a house is invariably owned by the matrilineal female descendants of the same ancestress; males have a moral claim to be sheltered by the female members of their maternal lineage, but do not pass on this prerogative to their children. By unilateral reckoning a father and his brothers inevitably belong to the same descent group, and correspondingly, a mother's sister must be of the same descent group as the mother. Thus the social structure yields the clear separation of patrilineal from matrilineal kinsfolk that appears in the terminology.

In reality the alternative hypothesis need not be mutually exclusive, for a tribe subdivided into unilateral groups may simultaneously favor the levirate and the sororate. A careful recent study on a statistical basis, however, definitely favors a primary correlation of the terminological features under consideration with unilateral descent.¹⁴ To put the matter in a nutshell and without the qualifications that a more technical exposition would require, the distinctive features of the Iroquois-Tamil system are distributed where unilateral descent prevails. They are largely absent where kinship follows our bilateral way of reckoning kindred. On the other hand, some American Indians with a bilateral family system like ours likewise have our way of designating uncles and aunts.

To present a further example of functional relationships established from distributional research, the Hopi Indians associate the following descriptively distinct features: children all belong to their mother's clan; a matrilineal group of women own the house; there is "matrilocal residence," i.e., a husband joins his wife's matrilineal kin instead of "patrilocally" taking her to *his* kin or establishing an independent home; terminologically, we not only find the features typical of the Iroquois, but also the relatively much rarer equation of one kind of cousin, the father's sister's son, with the father, both belonging to the same descent group. These traits do not occur in combination among the other tribes of the Shoshonean family nor with the same intensity among any other Southwestern people. But all these distinctive features

¹⁴ George P. Murdock, "Bifurcate Merging, a Test of Five Theories," *American Anthropologist*, XLIX (1947), 56-58.

reappear among the remote and totally unrelated Canella, a north-east Brazilian tribe of the Ge stock. What is more, the Sherente, linguistic relatives of the Canella and also residents of northern Brazil, display none of the items listed; they are patrilineal and patrilocal, and vest house ownership exclusively in males. A distributional study thus indicates that the rule of descent is organically related with certain other traits; eliminate one of them, and the others in the combination become less probable; introduce one of the set, and it favors the rise of the same correlates in tribes thousands of miles away from each other.¹⁵

One other instance must suffice. On my maiden field trip I discovered that a Lemhi Shoshone would never speak to his mother-in-law lest he be considered crazy. The following summer I found that the custom of mutual avoidance between these affinities was fairly widespread in the northern Plains. I argued the matter with a bilingual Assiniboiné, telling him that *we* conversed and sometimes even quarreled with our mothers-in-law, but was humiliated by a counter-thrust that could not be parried: "Well, white people will do *anything*!" A month or two later I had an ocular demonstration among the Crow Indians, where one day I found myself in the same tent with my interpreter, his wife, and her mother. The opportunity seemed ideal for pumping the bel-dame on the subject of ancient girls' games. However, my interpreter did not address my questions to the obvious source of enlightenment a few feet away, but to his wife, and she repeated them verbatim to her mother. The old woman then answered her daughter, who relayed the replies to her husband, through whose translation they ultimately reached me.

Now the distribution of this custom has haunted me for years. It exists in sundry parts of South as well as North America, Africa, Asia, Australia. On the other hand, it is sometimes intensely developed in a tribe and quite wanting among its congeners and neighbors. Thus, the Lemhi are pretty nearly alone among the Shoshonean stock in this respect, so that one naturally suspects their borrowing the taboo from nearby Plains tribes. However, the Hopi, fellow-Shoshoneans in the south, have not borrowed

¹⁵ Curt Nimuendajú, *The Eastern Timbira*, Univ. Calif. Publ. Amer. Arch. and Ethn., XLI (1946), 79, 83 f., 105, 125; *idem*, *The Serente*, pp. 16 f., 35.

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the custom from the Navaho, who are as intransigent in the matter as the Assiniboine. Corresponding instances could be easily multiplied. What an adequate theory must do is to account for both the cases of presence and of absence.

In Voltairean mood we might be tempted to apply the Darwinian principle in its most orthodox form. Surely a society that proscribes any social contact with one's mother-in-law would prevent all manner of stresses and strains. The nervous energy thus conserved by the males would give them an immense advantage over less enlightened neighbors, whom they would readily displace until they had conquered the earth. All that is required to lend glamor to the view is a good impressive label. As Goethe's Mephistopheles advises an eager freshman: "Mit Worten lässt sich trefflich streiten, mit Worten ein System bereiten." Now, what are dictionaries for? *Socrus* is Latin for "mother-in-law," let us then proclaim the socrusial theory of universal history. A carping critic might indeed object that so far the Caucasian has been able to survive alongside of the Lemhi Shoshone and the Crow, notwithstanding the Redskins' socrusially superior adaptation. But auxiliary hypotheses will take care of such arguments. Were Caucasian army officers, fur traders, voyageurs perchance hampered by the companionship of their mothers-in-law in early dealings with the savages? Had they suffered from these impedimenta, the story of the Far West and of colonial enterprise might well have been different!

My paternal sentiments, however, do not render me blind to other possible objections to the theory. It rests on the assumption that savages prohibit contact because of the hostility between son-in-law and mother-in-law. But this supposition is incorrect for every case on which we are properly informed. Avoidance is invariably explained as an expression not of animosity, but of extreme respect. Further, the taboo under discussion does not obtain uniquely between a man and his wife's mother, but often in the same societies embraces also the wife's father; and elsewhere strictly comparable rules are imposed on a woman and her father-in-law, indeed, on her and all her husband's elder kinsmen. What requires interpretation, then, is not an isolated quaint usage, but a whole category of more or less related rules of etiquette.

Let me admit at once that a thoroughly satisfactory solution is still wanting. But at least the way has been cleared for effectively *seeking* an adequate theory. It is interesting to compare the basically different interpretations offered by Tylor at different periods. In 1865 he concluded that so odd a usage could not have sprung up independently, but admitted that "the reasons for this avoidance are not clear." Twenty-three years later he goes beyond the explanation of the distribution and seeks, first, a correlation with other cultural phenomena; secondly, a reason for the taboo. On the basis of facts then available he correlates avoidance primarily with the mode of residence after marriage, finding statistical evidence for the proposition that in matrilocal residence avoidance is mutual between a man and his wife's parents, while patrilocalism involves a woman and her husband's parents. He sees the reason for this "cutting" of each other in the residents' wish to mark off the newcomer as a stranger. Tylor admits that this hypothesis is less secure than the empirical association of taboo and mode of residence.¹⁶

I am not concerned with the truth of either statement at present. What I should like to insist upon is that the humble facts of where cultural traits occur almost automatically impel toward seeking an *explanation* of the facts themselves. And when we try to understand the facts themselves, we are obliged to take into account other associated distributions. Many years ago I wrote the sentence: "When we do not know the distribution of a phenomenon, we know nothing that is theoretically significant." These are strong words, fighting words, and the late Professor Malinowski told me that they had infuriated him. But essentially I still regard the proposition as sound. If the differential calculus and chemical laboratories and skyscrapers were found indifferently in industrial civilization and among Australian blackfellows, these features would bear a meaning utterly different from their present one. Geographical distribution lies at the basis of our efforts at understanding culture.

¹⁶ Tylor, *Researches*, p. 287, 296; *idem*, "On a Method of Investigating the Development of Institutions, Applied to Laws of Marriage and Descent," *RAI, Journal*, XVIII (1899), 245-269.

Contemporary Trends in American Cultural Anthropology

DR. BEATTIE'S RECENT ARTICLE STIMULATES A COMPARABLE survey for America, even though I am obliged to repeat some reflections published elsewhere in recent years.¹ At the outset I wish to parallel a statement of Professor Firth's in answer to Murdock:² American ethnologists do not present a solid front but exhibit notable variations in outlook. I may add that several of them stand definitely nearer to some European scholars than to some of their compatriots.

It is not superfluous to suggest that anthropologists are not committed once and for all to a single type of orientation. Kroeber is an obvious example, but the proposition holds for much

Sociologus, V (1955), 113-121.

¹ J. H. M. Beattie, "Contemporary Trends in British Social Anthropology," *Sociologus*, V (1955), 1-14; R. H. Lowie, "Beitrage zur Völkerkunde Nordamerikas," *Mitteilungen aus dem Museum für Völkerkunde in Hamburg*, XXIII (1951), 1-27; *idem*, "Ethnography, Cultural and Social Anthropology," *American Anthropologist*, LV (1953), 527-534.

² George Peter Murdock, "British Social Anthropology," *American Anthropologist*, LIII (1951), 465-473; Raymond Firth, "Contemporary British Social Anthropology," *ibid.*, pp. 474-489.

younger men. Leslie White seems a different person when he defines Acoma culture and when he develops his evolutionary ideas. Mandelbaum's monograph on the Plains Cree and his publications on the Kota belong to different categories. Dr. Hortense Powdermaker cannot approach by the same methods the Melane-sians of Lesu, the Negroes of Mississippi, and the organization of Hollywood.

As these examples illustrate, there has been a notable shift of interest in geographical range. In 1925 the organ of the American Anthropological Association printed one article about the Hottentot, and its author was a foreigner; contributions dealt preponderantly with North America. On the other hand, recent issues of either the *American Anthropologist* or the *Southwestern Journal of Anthropology* are largely filled with reports on India, Pakistan, Central Asia, Japan, Micronesia, New Guinea, and Negro Africa. Herskovits must be credited with giving an impetus to Africanist research in the United States; Rowe is creating a corresponding concern with South America. The trend is correlated with the disintegration of most aboriginal North American cultures and with the opportunities afforded in the post-War II period. Where North American tribes have preserved aboriginal ways, as in Alaska and the Southwest, ethnographers continue to study them.

This expansion of outlook goes beyond the primitive peoples, embracing advanced civilizations, either *in toto* or with reference to specific segments ("communities") thereof. Some of the relevant work conforms to sociological patterns of research (as understood in the United States). Some of it falls into the category of applied anthropology (see below), as when McCorkle and A. Beals inquire into possible improvement of conditions in settlements on reclaimed land. At times the reformatory objective is implicit rather than explicit. In any case, community studies have become highly popular and are being adopted by French Canadians. Some of the investigations center in the description of the acculturation of ethnic minorities (see below), others stress the relations of folk and urban cultures. Noteworthy is the inclusion of Irish and Japanese settlements as objects of study.³

³ Marcel Riaux, *Description de la culture de l'île Verte*, Musée National du Canada, Bull. 133 (Ottawa, 1954); Alvin R. Beals and Thomas McCorkle, *Lost Lake*,

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Rather distinct in type are the researchers aspiring to a characterization of national ethos, a task often enough attempted by philosophers (Kant) and literary men (Taine). A book by Ruth Benedict may properly be mentioned here, for though unable to visit Japan, she employed Japanese informants in America. Her plan of correspondingly studying other nationalities has been pursued since her death by Margaret Mead, though as yet no other major report has appeared. Embree has concisely summarized Japanese life, and Haring has edited a symposium on the subject. Rodnick and Lowie have dealt with modern Germany.⁴

Though based on library research, a book of Kroeber's is best mentioned in this context.⁵ It considers the achievements of the advanced civilizations in art, literature, and science in order to illuminate the coexistence of outstanding talents at particular periods.

As to field techniques, some progress (though not enough) has been made in the learning of aboriginal tongues. Margaret Mead and Cora Du Bois, e.g., have learned the languages of some of the peoples they investigated in Melanesia and Indonesia, respectively; Douglas Haring and Edward Norbeck can converse in Japanese. Incidentally, there has been greater use of non-English sources; a reading knowledge of Spanish is common, and several of the younger ethnologists can read Russian.

Passing from regional to topical concentration, Wissler long ago realized that the effective exploitation of certain departments of culture required a specialist's training and engaged a textile expert to study aboriginal basketry. The experiment yielded meager results because of the expert's total lack of scientific background. Ethnographically valuable conclusions developed, however, when the textile specialist's preparation was coupled

Kroeber Anthropological Society Papers, No. 3 (Berkeley, 1950); Conrad M. Arensberg and Solon T. Kimball, *Family and Community in Ireland* (Cambridge, 1940); John F. Embree, *Suye Mura, a Japanese Village* (Chicago, 1939); Edward Norbeck, *Takashima, a Japanese Fishing Community* (Salt Lake City, 1954).

⁴ Douglas C. Haring, ed., *Japan's Prospect* (Cambridge, 1946); Ruth Benedict, *The Chrysanthemum and the Sword* (Boston, 1946); R. H. Lowie, *Toward Understanding Germany* (Chicago, 1954); David Rodnick, *Postwar Germans* (New Haven, 1948).

⁵ A. L. Kroeber, *Configurations of Culture Growth* (Berkeley, 1944).

with thoroughgoing ethnological training. Ceramic studies have profited from a corresponding synthesis and the application of techniques derived from the exact sciences.⁶

The case is, if possible, still stronger for musicology. We are fortunate in having had for some decades enthusiastic recorders and competent interpreters. Dr. Frances Densmore has been an unflagging pioneer, who incidentally obtained much information on other topics. She has been followed by Drs. H. H. Roberts, George Herzog, Bruno Nettl, and Richard A. Watermann.⁷

In contrast to many British colleagues, Americans regard social structure, too, as merely an important subdivision of Culture rather than as the theme of a distinct discipline beyond ethnography. We likewise have our specialists in this branch, though proportionately they are not so numerous as in England. Closest to the British "school" stands Radcliffe-Brown's one-time student, Fred Eggan, whose work on the Pueblos has been rightly acclaimed on both sides of the Atlantic. He and Spoehr both felicitously combine functional and sequential perspectives in the study of kinship. Independently, Murdock brings to the subject sociological, psychological, and sequential outlooks, and revives Tylor's statistical methodology in revised form (see also below). The traditional American orientation appears in Gifford's older survey of Californian kinship terminologies.⁸

Dr. Beattie seems somewhat unfair to his countrymen, thereby lessening the agreement between them and American investi-

⁶ Lila M. O'Neale, *Yurok-Karok Basket Weavers*, Univ. Calif. Publ. Amer. Arch. and Ethn., XXXII (1932), 1-184; *idem*, *Textiles of Highland Guatemala*, Carnegie Institution of Washington, Publ. 567 (1945); Florence M. Hawley, "Chemical Examination of Prehistoric Smudged Wares," *American Anthropologist*, XXXII (1930), 500 ff.

⁷ See Frances Densmore, *Teton Sioux Music*, BAE, Bull. 61 (1918); H. H. Roberts, *Musical Areas in Aboriginal North America*, Yale University Publications in Anthropology, No. 12 (New Haven, 1936); Bruno Nettl, "Text-Music Relationships in Arapaho Songs," *Southwestern Journal of Anthropology*, X (1954), 192-199.

⁸ Fred Eggan, *Social Organization of the Western Pueblos* (Chicago, 1950); Alexander Spoehr, *Camps, Clans and Kin among the Cow Creek Seminole of Florida . . .*, Field Museum of Natural History, Anthropological Ser., Nos. 1-4 (Chicago, 1941-1947); George Peter Murdock, *Social Structure* (New York, 1949); Edward W. Gifford, *Californian Kinship Terminologies*, Univ. Calif. Publ. Amer. Arch. and Ethn., XVIII (1922).

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gators, when he minimizes their concern with "the individual in society." The statement may hold for some, but assuredly not for all British guildsmen. If I understand Professor Firth correctly, it is this want in a skeletal inspection of society that he seeks to supplement by a study of "social organization." (Since my Oxford Dictionary defines "organize" as "give orderly structure to," I cannot approve of the phraseology, but I heartily concur in the idea.) When Firth shows us a rebellious Tikopian son temporarily flouting the claims of seniority and chieftainship, when Malinowski graphically depicts a Trobriand chief's conflict between paternal love and the submission to matrilineal law,⁹ are they not exhibiting precisely the individual as he reacts to society? I suggest that British social anthropologists have again and again intuitively thrown aside doctrinaire limitations and thereby given us a fuller understanding of the phenomenon they were describing. Certainly there remains a difference in emphasis, but no unbridgeable chasm between Americans and Britons in the study of society.

Intimately linked with the study of social structure is that of primitive law. Originally in collaboration with a professional jurist, Professor Hoebel has cultivated this field for years and recently produced a general book. His abundant reliance on case material harmonizes with the "personality in culture" approach.¹⁰

As to primitive economics, Herskovits and Foster have also shown intense interest in other subjects, hence cannot be regarded as strictly specialists. However, their work demonstrates the utility of treating a department of culture with an eye to the specialist's point of view.¹¹

One of the most obtrusive contemporary trends is the linkage of ethnography with psychology. In itself this dates back a long time. Boas, profoundly impressed by Francis Galton, was con-

⁹ Raymond Firth, *Elements of Social Organisation* (London, 1951), pp. 65-72; Bronislaw Malinowski, *Crime and Custom in Savage Society* (London, 1926), pp. 101 ff.

¹⁰ K. N. Llewellyn and E. Adamson Hoebel, *The Cheyenne Way, Conflict and Case Law in Primitive Jurisprudence* (Norman, Okla., 1941); E. A. Hoebel, *The Law of Primitive Man* (Cambridge, 1954).

¹¹ Melville J. Herskovits, *The Economic Life of Primitive Peoples* (New York, 1940); George M. Foster, *A Primitive Mexican Economy* (New York, 1942).

stantly harping on the individual variability of aborigines; and his insistence on the cultural determination of motor habits was of a piece with Marcel Mauss's discussion of "*les techniques du corps*." Even the concept of a cultural configuration transcending the sum of its elements had been anticipated by Haeberlin long before Ruth Benedict's celebrated book, and it was paralleled in England by Mrs. Aitken.¹²

As antecedent to contemporary practice may also be cited the collection of "personal documents," such as autobiographies, partial autobiographies, and diaries, by Paul Radin, Elsie Clews Parsons, and Truman Michelson.¹³

However, what we see today is veritable specialization to the nth degree. Margaret Mead, Cora Du Bois, Clyde Kluckhohn, Ralph Linton, Irving Hallowell, George Devereux, Jules Henry and others saturate themselves with technical theories of learning, undergo psychoanalysis or seek clinical experience, study word associations, apply Rorschach tests in the field, and contribute to psychiatric journals. Only a few samples of these publications can be cited here.¹⁴

These researches have naturally appealed to psychologists and psychiatrists, whereas those who regard Culture as the core of ethnological subject-matter have reacted in varying ways. Kroeber, while not hostile, prefers "describing cultural patterns . . . with a psychological suffusion" to "embarking on inquiries into the interrelations of culture with personality." White intransigently rejects all efforts to interpret culture on the psychological level.¹⁵ However, it seems clear that if the movement has

¹² Ruth Benedict, *Patterns of Culture* (New York, 1935); Herman K. Haeberlin, *The Idea of Fertilization in the Culture of the Pueblo Indians*, AAA, Memoirs, III (1916); Barbara Aitken, "Temperament in Native American Religion," *RAI, Journal*, LX (1930), 363-387.

¹³ For full references, see Clyde Kluckhohn, *The Personal Document in Anthropological Science*, Social Science Research Council, Bull. 53 (New York, 1945), pp. 79-173.

¹⁴ Margaret Mead and Gregory Bateson, *Balinese Character* (New York, 1942); Margaret Mead, "Some Theoretical Considerations on the Problem of Mother-Child Separation," *American Journal of Orthopsychiatry*, XXIV (1954), 471-483; Cora Du Bois, *The People of Alor* (Minneapolis, 1944); Clyde Kluckhohn, "Culture and Personality, a Conceptual Scheme," *American Anthropologist*, XLVI (1944), 1-29; George Devereux, *Reality and Dream* (New York, 1951).

¹⁵ Leslie A. White, *The Science of Culture* (New York, 1949); A. L. Kroeber, in Sol Tax et al., *An Appraisal of Anthropology Today* (Chicago, 1953), p. 357.

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done nothing else, it has helped to give system and precision to studies of primitive childhood, an unchallengeably legitimate part of ethnographic data.

To turn to a branch of ethnography that demands less exacting preparation, folklore has for decades claimed the enthusiastic support of American anthropologists in distinctive fashion. Apart from gathering vast bodies of raw material, they have studied the geographical range of motifs and plots, interpreted the distribution in terms of historical connections, and inquired into correlations between the tales and the actual life of the tellers. Some scholars (Radin, Jacobs, Benedict) are not averse to psychoanalytic interpretations. Even more characteristic is the attention paid to literary style, a bent correlated with Boas's insistence on the collection of texts in the vernacular.¹⁶

Cultural change has been studied for a considerable time both in Europe and America, so that "cultural dynamics" has become a veritable catch phrase. In the United States metamorphosis from internal causes has not been ignored, but the emphasis has been on acculturation, i.e., on changes due to contact with alien groups. The large number of distinct ethnic units in the United States naturally prompted these investigations, in which Herskovits, Linton, Redfield, Ralph Beals, Mandelbaum, Barnett, Keesing, Greenberg, and others have participated in their several ways. Such researches may coincide with "community studies" and also easily merge in consideration of the personality of the acculturating individual.¹⁷

Applied anthropology has also come to the fore. Some of its votaries correspond to the ethnological advisers of British and Dutch colonial administrations. In fact, the Bureau of American Ethnology was founded in 1879 with the explicit purpose of aiding the Government in dealing with Indians. Actually it soon

¹⁶ Paul Radin, *Winnebago Hero Cycles: Study in Aboriginal Literature*, Indiana University Publications in Anthropology and Linguistics, Memoir I (Baltimore, 1948); *idem*, *The Culture of the Winnebago as Described by Themselves*, *ibid.*, Memoir 2; *idem*, *The Evolution of an American Indian Prose Epic* (Basel, 1954); Gladys A. Reichard, *An Analysis of Coeur d'Alène Indian Myths* (Philadelphia, 1947); R. H. Lowie, *Studies in Plains Indian Folklore*, Univ. Calif. Publ. Amer. Arch. and Ethn., XL (1942), 1-28.

¹⁷ For bibliography, see Melville J. Herskovits, *Man and His Works* (New York, 1948), pp. 523-541.

turned into a pure research institute, although contact with the Bureau of Indian Affairs was resumed in 1934. Altogether different are the efforts of Dr. Chapple and his followers, who for practical purposes analyze interpersonal relations in business and industry. Different again is the attempted synthesis of educational and anthropological insights, as debated at a joint conference of ethnologists and pedagogues in 1954.¹⁸

With so much energy lavished on other subjects, the traditional concerns of ethnography are bound to suffer. Thus, considering the enormous number of professional anthropologists, there have been very few comprehensive accounts of tribal life during the past twenty years. Spier's admirable treatises on the Havasupai, the Klamath, and the Gila River Yumans fall into the period just preceding. Kroeber's vivid accounts of the Mohave and Yurok, printed in 1925, were written five years earlier. In consonance with the specialization mentioned above, even monographs preponderantly following the older pattern of presentation tend to limit their scope to particular departments of social life—witness Morris Opler's and Goodwin's books on the Apache, Reichard's on the Navaho, E. C. Parsons' on the Pueblos. Thus, notwithstanding the enormous material gathered on the Plains and the Southwest, we have not yet a thorough account of Dakota ("Sioux") or Hopi or Navaho life. Nevertheless, such publications as those mentioned indicate that the old ethnographic patterns are by no means extinct, and apart from the slighting of technology such recent monographs as Flannery's and Drucker's or the somewhat older work of Mandelbaum and Lantis show a high degree of competence.¹⁹

¹⁸ Elliot D. Chapple, "Applied Anthropology in Industry," in A. L. Kroeber, *Anthropology Today* (Chicago, 1953), pp. 819-831; Edward A. Kennard and Gordon MacGregor, "Applied Anthropology in Government: United States," *ibid.*, pp. 832-840; George D. Spindler, ed., *Education and Anthropology* (Stanford, 1955).

¹⁹ Leslie Spier, *Havasupai Ethnography*, AMNH, Anthropological Papers, XXIX (1928), 81-392; *idem*, *Klamath Ethnography*, Univ. Calif. Publ. Amer. Arch. and Ethn., XXX (1930), 1-338; *idem*, *Yuman Tribes of the Gila River* (Chicago, 1933); Morris E. Opler, *An Apache Life-Way: The Economic, Social and Religious Institutions of the Chiricahua Indians* (Chicago, 1941); Grenville Goodwin, *The Social Organization of the Western Apache* (Chicago, 1942); Elsie Clews Parsons, *Pueblo Indian Religion* (Chicago, 1939); Gladys Reichard, *Social Life*

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An even sharper break with the past appears in the aversion from distribution studies of the type developed by Wissler and Spier. The last ambitious effort in this line was probably Kroeber's Culture Element Survey west of the Rocky Mountains (completed in 1938).²⁰ This sort of thing is simply no longer done, nor is it fashionable to indulge in historical reconstruction.

But though not fashionable, the old problems do not disappear, for they obtrude themselves at every point unless one deliberately ignores them. Thus, it is true that a whole volume of the report on the International Congress of Americanists in 1949 was devoted to Acculturation and relatively little space to questions in vogue a generation ago. Yet the relationship of Mexican to Peruvian metallurgy, of moieties in the simpler and the advanced South American cultures, of South Asiatic and Middle American civilizations were again under consideration.²¹

This historical bent is strengthened by the frequent tie-up of ethnography with archaeology and documented history. All three disciplines are combined in model fashion by Rowe in his South American work, while Wedel and Strong excavate, so far as possible, historically authenticated sites, working backwards from the known to the unknown and with the aid of written records. Apart from archaeology, there is to be noted a commendable zeal in exploiting historical sources by Rowe, by his students, by Fenton, and by Ewers. An example for this type of work was set by the self-sacrificing labors of Swanton in extracting ethnographic data from the old chroniclers of the Southeastern United States.²²

of the Navaho Indians (New York, 1928); *idem*, *Navaho Religion* (New York, 1950); Regina Flannery, *The Gros Ventres of Montana, Part I: Social Life* (Washington, 1953); Philip Drucker, *The Northern and Central Nootkan Tribes*, BAE, Bull. 144 (1951); Margaret Lantis, "The Social Culture of the Nunivak Eskimo," *Transactions of the American Philosophical Society*, n.s., XXXV (1946), 151-323; David G. Mandelbaum, *The Plains Cree*, AMNH, Anthropological Papers, XXXVII (1940), 155-316.

²⁰ For exposition of its purposes, see A. L. Kroeber, *The Nature of Culture* (Chicago, 1952), pp. 263 ff.

²¹ Sol Tax, ed., *Selected Papers of the XXIXth International Congress of Americanists: The Civilization of Ancient America* (Chicago, 1951), pp. 219-223, 299-309; *idem*, ed., *Indian Tribes of Aboriginal America* (Chicago, 1952), pp. 1-7.

²² Waldo Wedel, "Culture Sequence in the Central Great Plains," *Smithsonian Miscellaneous Collections*, C (1940), 291-352; William D. Strong, "From History

Finally, a neo-evolutionist tendency has cropped up. Leslie White, an avowed follower of L. H. Morgan, whose thinking shows some affinity with V. Gordon Childe's, defines cultural progress in terms of the way "in which they harness energy . . . to serve human needs." However, neither White nor Lesser and Jacobs—two of Boas's students, incidentally—, who have tried to rehabilitate evolution, believe in unilinear development. They merely suggest determining the necessary preconditions of particular phenomena. While these three writers merely suggest a programme, at least two other scholars have passed to concrete formulations—a natural consequence of their establishing functional relationships, since the priority of *one* of two or more correlates implies a "causal" sequence. Murdock has applied this reasoning to the development of kinship systems. Steward emphasizes the parallelism due to ecological factors; recently he has attempted establishing regularities in the rise of advanced civilizations.²³

As stated in the beginning, there is thus no single-mindedness among contemporary American ethnologists; further, the same man may simultaneously combine varied interests and methods or may shift his allegiance in the course of his professional career. But, in contrast to the American past and to European practice, it may be said that the distinctive contemporary trends in the United States are marked by interest in all types of culture, primitive or advanced; enthusiasm for community and acculturation studies; emphasis on "personality and culture"; some urge to

to Prehistory in the Northern Great Plains," *ibid.*, pp. 353–394; William N. Fenton, "Problems Arising from the Historic Northeastern Position of the Iroquois," *ibid.*, pp. 159–251; J. R. Swanton, *The Indians of the Southeastern United States*, BAE, Bull. 137 (1946); John C. Ewers, *The Horse in Blackfoot Indian Culture*, BAE, Bull. 159 (1955); John H. Rowe, "Inca Culture at the Time of the Spanish Conquest," in J. H. Steward, ed., *Handbook of South American Indians*, BAE, Bull. 143 (1946), II, 183–330.

²³ L. A. White, "Energy and the Evolution of Culture," *American Anthropologist*, XLV (1943), 335–356; Alexander Lesser, "Evolution in Social Anthropology," *Southwestern Journal of Anthropology*, VIII (1952), 134–146; Melville Jacobs, "Further Comments on Evolutionism in Cultural Anthropology," *American Anthropologist*, L (1948), 564–568; Murdock, *op. cit.*, pp. 184–259; J. H. Steward, *Basin-Plateau Aboriginal Socio-political Groups*, BAE, Bull. 120 (1938); *idem*, "Cultural Causality and Law: A Trial Formulation of the Development of Early Civilizations," *American Anthropologist*, LI (1949), 1–27.

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rehabilitate evolution (though not of the unilinear brand); a revision of distributional research and of historical reconstruction. The last-mentioned type of investigation continues to be pursued by a fair body of workers, even though they do not hold the center of the scene.

Berthold Laufer as Ethnologist

IN RECENT WEEKS I HAVE HAD OCCASION TO USE SOME OF Laufer's writings and have been impressed anew with the value of his work for anthropology, as well as with his vast learning and his unexcelled judgment. He turns from the history of spectacles to Hellenistic folklore, from Ainu numerals to cricket-fighting, or from Chinese symbols to the domestication of reindeer. Since I sometimes meet younger anthropologists who consider him exclusively as a sinologue, I venture to give a brief perspective upon him as an ethnologist.

Laufer was an anthropologist, not because he did some routine ethnographical research on the Amur River, but because he used his vast apparatus of learning as a means to the end of elucidating the development of culture—turning it upon one subject after another. It is greatly to his credit that he preserved a certain largeness of view and that, deliberately cutting loose from the guild of sinologues and orientalists, he has become the culture-historian par excellence among investigators of higher cultures. His inquiries may be ostensibly directed toward a minute subdivision of the total subject, but he almost invariably brings his

This paper was found among Professor Lowie's unpublished MSS. Internal evidence suggests that it was written after 1950.

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data into connection with the procession of cultural phenomena in general. Thus, a monograph on *Jade* (1912) brings up to date what was known of a Stone Age in China; in a treatise on *Chinese Clay Figures* (1914) significant passages shed light on the diffusion of ironwork in Eastern Asia; while the essay on *The Beginnings of Porcelain* (1917) contains a sketch of the origin and dissemination of the potter's wheel.

Some of Laufer's broader interpretations are intimately connected with Eduard Hahn's system of ideas, so that a brief reference to these views is indispensable. This German scholar is one of the most suggestive and most irritating writers in the domain of culture history. More than anyone else he has dispelled the hoary misconception that tillage was always preceded by stock-breeding. Hahn has also given currency to the useful distinction between hoe-culture (gardening) and plow-culture (agriculture). Moreover, he has unremittingly emphasized the nonutilitarian motives in earlier and cruder countries. In the main his contribution to anthropological theory is considerable, but intertwined with truly liberating ideas there are fantastic speculations and positive errors. Hahn carelessly states that almost all primitive people till the soil with hoes; he is capable of denying that the Maori of New Zealand had dogs—presumably confounding them with the Tasmanians. Having once decided that the dromedary is not a distinct species but merely a form of domesticated camel, he completely disregards evidence to the contrary. What is more important, he nowhere considers the objections made by Hatt, Schmidt, and Koppers against one of his pet ideas—the impossibility of domestication of livestock by people in the hunting stage. The scholars mentioned hold that wild game animals impounded by nonhorticultural tribes in communal drives might breed in the semi-captivity of a large enclosure; and whether the argument is convincing or not, it certainly merits discussion, for with its cogency is bound up the question of the beginnings of stock-breeding.

In short, Hahn is a stimulating but slovenly and opinionated writer, who cannot be regarded as an ultimate authority, and while Laufer is surely not an uncritical disciple, he evinces for

him that curious selective affinity which at times unites the most antithetical temperaments. Specifically, Hahn's theory as to the sexual division of labor has led him astray—as it led me, also. For Hahn's oft-reiterated assertion that in ruder cultures women hoe the land is an egregious misstatement. It does not hold in literally dozens of cases—in America, in Polynesia, and in Africa. Hahn's correlation must accordingly be rejected, but it involves in its fall a rather important corollary drawn by Laufer. If, he argued, the plow is used by men, while women invariably wield the hoe, agriculture cannot be genetically connected with horticulture and the plow cannot be derived from the hoe.¹ But since the premises are unsound, the inference is eliminated. The origin of the plow is of necessity a technological problem, but it is certain it did not arise spontaneously out of nothing. However, if Hahn is responsible for Laufer's inferences on this point, he may also be credited with having inspired one of Laufer's happiest generalizations—the correlation of the potter's wheel with the male sex and of handmade pottery with women. It is also likely that Laufer's sane aversion to rationalistic psychologizing was at least intensified by Hahn's example.

Laufer is, however, not in the least averse to generalizing. Nothing is more admirable than the firm strokes with which he marks out the geographical distribution of single traits and their combination into complexes. He shows us that alliteration, assonance, epic poetry, and dairying techniques occur together among Mongolic, Turkic, and Finno-Ugrian peoples; that in Chinese textiles hemp assumes the part played by flax in the Mediterranean region; that Far Oriental animal husbandry is without the dairying processes that loom so large in the lives of the pastoral nomads and the higher civilizations; that the potter's wheel characterizes the Old World civilization in contrast alike to the ruder techniques of the Eastern Hemisphere and to all the cultures of the Western. From such determinations there emerge at times clearly demarcated culture areas: the North Chinese, e.g., are planters of wheat, barley, and millet, harness

¹ Eduard Hahn, *Die Entstehung der Pflugkultur* (Heidelberg, 1911), p. 9; articles on Ackerbau, Hund, Haustier, Kamel, in Max Ebert's *Reallexikon der Vorgeschichte* (Berlin, 1924-1932).

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the ox to a plow, travel over highways on mounts or in wheeled carts; the South Chinese grow rice with the aid of the water buffalo and transport goods and persons by boat or in sedan chairs.²

As might be expected, Laufer is *historical* in a stricter sense than most anthropologists. This tendency is well illustrated in the two classical monographs on "The Beginnings of Porcelain in China" and "The Reindeer and Its Domestication."³ To begin with the earlier paper: What is porcelain? It is a form of glazed pottery the body of which consists of two ingredients fired together, kaolin and petuntse. Kaolin alone cannot make porcelain, for it produces vessels that are "porous, fragile, and opaque. Petuntse alone softens in the kiln, and runs together into a lump." Hence a culture-historical inquiry must start with the occurrence of these two elements together on Chinese soil. Laufer points out the archaeological evidence for neolithic hand-made pottery there and shows it was superseded by wheel-made ware from the earliest period of written records. This later form of the industry was shown to be genetically related to the early civilization of Western Asia, i.e., "the ancient Chinese wheel has sprung from the same source as that found in the West. Both are identical as to mechanical construction, even in minor points, and as to effect." Glass and glazing, however, although known for millennia in Egypt, did not appear in China until much later. The Chinese did not begin to glaze pottery until the Han dynasty (206 B.C.-220 A.D.), developing the practice especially during Wu's reign (140-87 B.C.). Chemical analysis shows that toward the end of the dynasty the Chinese were able to produce a ware having the chemical composition of true porcelain but not yet its characteristic physical features. By continuous experimentation extending over several centuries the deficiencies

² Berthold Laufer, "Skizze der Mongolischen Literatur," *La Revue Orientale*, 1907, p. 241; *idem*, *The Beginnings of Porcelain in China*, Field Museum Anthropological Ser., XV (1917), 150; *idem*, "The Fundamental Ideas of Chinese Culture," *Journal of Race Development*, V (1914), 160-174.

³ The conclusions of the latter paper have been challenged by Hatt, Koppers, and Bogeras as assigning an inadequate antiquity to the cultural phenomena discussed. But apart from the fact that these objections seem only partly justified, we are here concerned less with the correctness of the interpretation than with the aims and principles of historical research as conducted by Laufer.

were overcome, and in the seventh century A.D. these efforts culminated in the production of genuine white porcelain.

The same desire for sharp definition of cultural origins appears in the discussion of the reindeer, an animal that occurs wild in the wooded Ural and Baikal regions no less than in the northern Eurasian tundra; and it was never domesticated by the American aborigines, though they constantly hunted the wild animal. These two distributional facts serve for a general orientation. If the domestication of the species is restricted to the Old World, it is not presumably of great antiquity, in view of the known cultural exchanges between Arctic America and Siberia. And if the reindeer exist about Lake Baikal, where a variety of other domesticated species occur, the idea of domestication may have been simply extended to the reindeer at a relatively late date. Thus, riding reindeer-back, practiced by the Soyot and the Tungus, is an imitation of already familiar horseback-riding; the sporadic milking of reindeer is the transfer of a technique developed with cows, mares, and sheep; the reindeer-sledge is based on the dog-sledge. Accordingly, we are dealing essentially with a derivative cultural feature. This would naturally place reindeer domestication at a later date than that of cattle, horses, and dogs. The Chinese annals of 499 A.D. describe a people identifiable with the modern Soyot around Lake Baikal who kept horses, cattle, and reindeer. But since the reindeer were used in imitation of these other species, the ancestral Soyot fulfil all the conditions that must be imposed on a putative tribe of first reindeer domestication. Hence, Laufer refers the original process to this region and, allowing five centuries for its completion, dates the origins back to the beginning of our era.

Reliance upon actual history fosters that peculiar quality of mind known as "historical-mindedness." The recorded sequence of events is often in glaring contradiction to what seems a priori possible. It scoffs not only at unilinear evolution but at *simpliste* diffusionism as well. Laufer is saturated with these principles, and his writings teem with illustrative materials. Let us cull a few samples.

Chinese viticulture is of admitted antiquity, and there is a

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species of grapevine that is indigenous to China; what more plausible than to assume a native origin of Chinese viticulture? But the Chinese records demonstrate that the native species was never grown and that the first knowledge of the cultivated vine reached the Middle Kingdom through General Chang K'ien's travels to Fergana and Bactria (126 B.C.). Moreover, while the Chinese received the grape from an Iranian people at this period, they acquired the art of wine-making as late as 640 A.D. from a Turkish tribe of Turkestan.⁴

Again, when we learn that both the Chinese and the nomads of the North wore narrow, long-legged riding-boots, can we doubt that the invention is due to the higher civilization and was then passed on to the ruder people? But once more the Chinese chronicles give a clear-cut reply contrary to the expected one: as a military counterstroke against the inroads of hostile Northerners, the Chinese under Wu-ling (325-299 B.C.) adopted the tactics of mounted infantry from their enemies, borrowing their riding costume at the same time. To quote our author: ". . . even a higher civilized nation may take lessons in clothing itself from peoples regarded as barbarians."⁵

And once more, no fact is more firmly established than the far-reaching influence exerted by the Chinese on the ancient civilization of Japan. Accordingly, the inference would seem reasonable that the sarcophagi of the latter were derived from the usual source, but once more actual history leads to the reverse of the a priori probable assumption.⁶

No wonder that Laufer sets his face against not only the facile evolutionism of the "classical" period but likewise against an equally schematic diffusionism. No one is more keenly impressed by the uniqueness of cultural happenings, and nowhere will the reader find more telling instances of that false convergence which has been misleading to theorists of both schools. When sarcophagi turn up in Egypt, China, and Japan, Laufer neither asks by what uniform stages "*the* sarcophagus" has

⁴ Laufer, *Sino-Iranica*, Field Museum Anthropological Ser., XV, No. 3 (1919), 221, 223.

⁵ *Idem*, "Moccasins," *American Anthropologist*, XIX (1917), 298.

⁶ *Idem*, "Chinese Sarcophagi," *Ostasiatische Zeitschrift*, I, 318-334.

evolved nor from what center it has been distributed. He points out that the catchword covers a variety of varying conceptions, the sole resemblance lying in the existence of a stone container for burial. Similarly, he likes to demonstrate the distinct paths leading to comparable results in different areas. Painting, in China, evolved in close connection with calligraphy, while in ancient India there is no suggestion of such a relationship but rather an influence of linear craftsmanship inspired by physiognomic lore.⁷

But the finality of historical research, in the strict sense of the term, carries with it an important implication. Probably never intent upon abstract formulations as such, Laufer shed a flood of light upon those processes of cultural growth and decay which others place overtly in the foreground of discussion, often without rising beyond sterile-word-chopping. To revert to the two monographs summarized above; both illuminate the cardinal *psychological* problems of borrowing and invention. Even though reindeer-breeding was originally an application of processes developed in connection with the dog, horse, and ox, the fact that these processes were extended to a new species puts this case into quite another category from the mere acquisition of reindeer already domesticated from another tribe by purchase or theft. For in the former sense there is that creative expansion of a fruitful idea, that progression from the known to the unknown, which is the characteristic of a genuine invention. Similarly, with porcelain, except that here the hypothetical elements approach zero. Glaze is transmitted from the West, but the phenomenon is not one of passive acceptance. The receivers are stimulated into original technical experimentation and this results in the creation of as novel a product as any in the history of invention. What men like Boas and Rivers have discussed in the abstract—the impulse to new achievements derived from borrowed cultural elements—is brilliantly exemplified by Laufer's historical analysis. Laufer's writings, severely historical in form and containing only incidental references to theory and methodology, are a veritable treasure-trove for the student interested

⁷ *Idem, Documente der indischen Kunst*, Leipzig (1913), pp. 31, 192.

Berthold Laufer as Ethnologist

in the dynamics of culture. No other author so plainly demonstrates how factitious is the opposition of history and psychology. If we want to know the real motivation and rationale of cultural events, we cannot do better than to find out *what* the events really are and *how* they actually succeeded one another.

The Development of Ethnography as a Science

LIKE OTHER SCIENCES, ETHNOGRAPHY HAS A LONG PRE-scientific past. Even savages could not help noting that alien groups had tools, weapons, and customs different from their own. Australian aborigines, obliged to get their diorite for axe-heads from distant parts of the continent, met fellow-blacks with strange ways of life, some of which they rejected while eagerly adding others to their own inventory. On a higher level, Herodotus (490–424 B.C.) not only noted what he saw in Egypt and Babylonia, but systematically recorded his observations, thus becoming the father of ethnography no less than of history. Comparable information was gathered outside the Occidental civilizations. The Chinese General Chang K'ien, who set out on a mission to the west in 138 B.C., returned in 126 B.C. and “submitted to his astonished countrymen a glowing account of the new world which he had discovered, and which was nothing less than the Hellenic-Iranian civilization inaugurated in those regions by the successors of Alexander the Great.” Chinese scholars generally registered the customs of their neighbors. For

This paper was found among Professor Lowie's unpublished MSS. Internal evidence suggests that it was written after 1953.

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example, they tell us that in 307 B.C. Wu-ling, king of Chao, adopted the cavalry tactics of the northern nomads; and that even the ruler of Tibet abandoned felt dress only as late as 641 A.D.¹

The Middle Ages must be credited with outstanding accessions to ethnographic knowledge. To cite only two illustrious names, there were the narratives of Ibn Batuta (1304–1377) and of Wilhelm Rubruk or Ruysbroeck (*ca.* 1210–1270). The Arab traveler, among other things, describes the shocking liberty enjoyed by women of the Tuareg people in the Sahara; the Fleming graphically pictures the economic life, divinatory practises, and what not of the Mongols.

Excellent as were many of these early accounts, they represented even jointly only raw material for a prospective science so long as the majority of human societies remained literally unknown and the known ones accordingly could not be seen in perspective. A zoölogist would have a curious conception of the animal kingdom if he knew only about mammals and arthropoda. In this respect the mere discovery of new territories—say, of Australia in 1605—was of no direct moment. But in America the first contact with Indians was soon followed by full reports on their arts, manners, and beliefs—witness Fray Bernardino de Sahagun's exhaustive account of Mexican religion, based on inquiries probably started about 1530. In the South Pacific, Captain Cook, himself a good observer, took with him such scientifically trained men as Georg Forster, Johann Reinhold Forster, Sir Joseph Banks, and Dr. Solander, the "ingenious and learned Swede" and "disciple of Linnaeus." From the new data on American, Polynesian, and Melanesian natives provided by naturalists, missionaries, and traders a respectable body of knowledge for the major divisions of the globe had accrued before the middle of the last century. In 1843 Gustav Klemm thus ventured to publish his *Allgemeine Cultur-Geschichte der Menschheit*, soon to be supplemented by a *soi-disant* treatise on *Allgemeine Culturwissenschaft*.

¹ Berthold Laufer, "The Fundamental Ideas of Chinese Culture," *Journal of Race Development*, V (1914), 160–174; *idem*, "The Early History of Felt," *American Anthropologist*, XXXII (1930), 1–18.

Educated reporters automatically went beyond their descriptive findings, for these recalled things they had read or themselves seen elsewhere. The Jesuit Joseph François Lafitau (1670–1740) has been, with considerable justice, pronounced an outstanding precursor of ethnographic *science*. Having spent five years in eastern Canada and profited from the more prolonged experiences of a fellow-missionary, he did not content himself with describing what he had learnt, but systematically looked for parallels in classical antiquity, trying to illuminate Indian by Greek or Roman customs, and vice versa. That today his comparisons often strike us as odd is not to be wondered at, but Lafitau's *Moeurs des sauvages américains, comparées aux mœurs des premiers temps* (Paris, 1724) certainly does not merit the depreciatory comment by a recent historian, who regards the work as merely “a very generalized rehash of the *Jesuit Relations*.”²

In the sequel all sorts of wild theories developed from analogies stressed by otherwise meritorious observers. Suffice it to mention but two. According to George Catlin (1796–1872), the Mandan Indians of the upper Missouri were descended from Welsh immigrants because they crossed rivers in hide-covered tub-like boats (“bull-boats”) resembling the Welsh coracles. Again, James Adair in *The History of the American Indians* (London, 1775) identified the New World aborigines with the Ten Lost Tribes of Israel because Jews and Indians shared menstrual and dietary taboos. On this theory Swanton has offered the obvious comment: “Taboos were so numerous with the old-time Indians that parallels with the taboos of any other nation would be found without a great deal of difficulty.”³ As for Catlin's conclusion, bull-boats would prove Tibetan as well as Mandan affinity with the Welsh. Ignorance of geographical distribution underlies many fallacious conclusions—then and later.

² Wilhelm E. Mühlmann, *Geschichte der Anthropologie* (Bonn, 1948), pp. 41–44; Wilhelm Schmidt and Wilhelm Koppers, *Völker und Kulturen* (Regensburg, 1924), p. 20; W. Vernon Kienitz, *The Indians of the Western Great Lakes, 1615–1769* (Ann Arbor, 1940), p. 333.

³ John R. Swanton, “Religious Beliefs and Medical Practices of the Creek Indians,” *BAE*, 42d Ann. Rept. (1928), pp. 477–672.

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By no means all of the earlier suggestions, however, belong to the realm of fancy. Scientists, automatically applying their accustomed professional procedures, collated their own findings with their predecessors', whether in the same tribe or elsewhere, and often drew sound conclusions or broached basic problems. Thus Georg Forster inquired into the effect of climate on a people's manners and ethos; further, human irrationality struck him forcibly when he beheld nude Yahgan Indians ornamenting their shivering bodies. The shortcomings of man's intellect also impressed Alexander von Humboldt when he contrasted the restricted range of the pre-Columbian potato with the early and wide diffusion of tobacco. Racial psychology engaged the attention of Humboldt and Prince Maximilian of Wied-Neuwied, both of whom conceived a favorable opinion of aboriginal American intelligence. Having heard a young Carib preach, Humboldt concluded that the orator's stock was capable of high cultural development (*ein begabtes, einer hohen Kulturentwicklung fähiges Volk*).⁴

Nevertheless, even the best-trained scientists of the earlier periods could not fully attain the ends of an ethnographic science for the simple reason that the central aim of such a science had not yet crystallized. As Kroeber and Kluckhohn set forth in a recent monograph,⁵ the concept of culture—the sum total of a man's social heritage—had indeed been variously adumbrated, but not clearly envisaged before Klemm, and not rigorously circumscribed before Tylor's classic definition in 1871. Then at last it became obvious that culture embraced everything transmitted not by biological heredity, but by membership in a social group,—not merely houses, crafts, and food-gaining pursuits, but also social structure and usage, religious ceremonial and belief, folk-tales and amusements. This totality could no longer be adequately studied by the way, but required a specialist's concentration.

⁴ Hermann Hauff, *Alexander von Humboldt's Reise in die Aequinoctial-Gegenden des neuen Continents* (Stuttgart, 1862), VI, 124, 282 f.; Jacques Moleschott and Georg Forster, *Der Naturforscher des Volks* (Frankfurt am Main, 1854), pp. 47, 57.

⁵ A. L. Kroeber and Clyde Kluckhohn, *Culture: A Critical Review of Concepts and Definitions*, PMH, Papers, XLVII, No. 1 (1953), 10, 24 f., 35 f.

As a matter of fact, if a geographer or biologist could not do justice to the whole range of these phenomena, neither could an ethnographer. It developed that the aims of science could be best served by further specialization and that certain phases of culture could be adequately investigated only through collaboration. On the Cambridge Expedition to Torres Straits organized by A. C. Haddon in 1898 each participant devoted himself to aspects of native life for whose study he was specially prepared: Haddon, a zoölogist, took up material culture; Rivers, a psychologist with special interest in social life, gave psychological tests and applied himself to the intricacies of kinship; and so forth. Generally speaking, a full ecological picture implies the coöperation of a geographer, a botanist, and a zoölogist; the songs, a feature often of great moment to the natives, must be transcribed and comparatively dealt with by a musicologist; the very nature of whatever metal work may be observed can be determined only by a metallurgist. To take the last instance, an ethnographer wants to know how the metal work of aboriginal Columbia compares with that of Peru. Only a trained expert can clarify the cultural issues: did the Peruvians achieve bronze by deliberately alloying copper with tin? If so, what were the relative proportions? Did the Colombian "tumbaga" rival the Peruvian bronze in hardness and efficiency? For the answer the ethnographer obviously must depend on the chemist and metallurgist.⁶

Indeed, the ethnographer must at times rely on the aid of laymen. Since he is rarely able to spend more than a limited time with the subjects of his study, various phases of native life elude him. An intelligent missionary, trader, or squaw-man who speaks the native language may be superior in some respects to the greatest scientist. In December, 1832, Charles Darwin encountered the Tierra del Fuegians, leaving us a good report so far as it goes. But beyond externals he could *not* go. Says he: ". . . it was singularly difficult to obtain much information from them concerning the habits of their countrymen . . . it was generally impossible to find out, by cross-questioning, whether

⁶ Erland Nordenskiöld, *Comparative Ethnographical Studies* (Göteborg, 1931), IX, 101-112.

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one had rightly understood anything which they had asserted.”⁷

On the other hand, the most intelligent outsider or even an ethnographer who is not abreast of recent developments in his science is bound to fall short of the goal. To cite an egregious illustration, excellent lay writers have described “uncles” and “cousins” as fulfilling certain social duties, whereas the veriest novice today would determine the exact nature of the relationships involved. Some scientific problems defy solution precisely because observers who once had the opportunity to record certain facts neglected to do so. An approximation to an ideal solution lies in the close collaboration of a trained investigator with an alert permanent resident, the former directing the latter’s attention to points otherwise missed, the latter supplying what can come only from a prolonged sojourn.

Because the ethnographer deals with the total range of human activity as socially determined, he at times puzzles the votaries of other scientific disciplines. Sometimes they have raised the issue whether students of, say, folk-tales ought to rank as “scientists” at all; I remember a relevant challenge at a meeting of the National Academy. The answer is obvious. Science must take cognizance of the whole of reality. The individual scientist does not control the segment of reality he has chosen for his subject matter; his business is to investigate it in all its manifestations. The paleontologist who finds a fossil human skeleton does not ignore the stone artifacts beside it; and the ethnographer, whatever his personal preferences, cannot on principle ignore aspects of man’s social existence that happen to have hitherto been dealt with by humanists or even by amateurs. The point is that when *he* deals with them he approaches them as an objective investigator. Of course, he should be able to share the native’s thrill over an effectively told narrative; but as a scientist he must do more. He must analyze it, compare it with similar tales elsewhere, determining its distribution, must establish its function,—whether it is told for sheer aesthetic entertainment or as a sacred myth, whether it appears in several versions, and so forth. If humanists have devised suitable techniques for such purposes, the ethnographer will naturally make them his own.

⁷ Charles Darwin, *Journal of Researches* (London, 1845), chap. x.

Put more broadly, the ethnographer in principle strives to learn everything he can about all phases of culture; and in order to do this he is naturally obliged either to devise appropriate techniques for study or to utilize the services of experts from other branches of learning. The resulting attention to minutiae inevitably made much of ethnography unpalatable to the general public. Indeed, it became boresome for the ethnographer himself so far as he was not specifically interested in certain subjects. A description of textile weaves or a roster of kinship terms is deadly—unless it is vivified by a sense of problem. The justification for delving into these details lies in the fact that they can shed light on such broader issues as human inventiveness and questions of genetic affinity.

To illustrate concretely what was involved in the coming of age of ethnography as a science, we may consider the treatment of certain crafts by Georg Schweinfurth in the early 1870's. Schweinfurth, a justly famous naturalist and explorer, was particularly interested in technology and on his most celebrated expedition he paid special attention to the Mangbettu (Monbuttoo) in what is now the northeastern Belgian Congo. Yet all he notes about the pottery of this people is that it is hand-made, superior to that of other African Negroes, uniformly spherical, and except in a unique specimen lacks handles, yet is ornamented with raised patterns that facilitate manipulation. By way of contrast, in the beginning of the present century Mr. and Mrs. Routledge, amateurs themselves though in close contact with professionals, visited an East African tribe and in their report devoted five printed pages and ten plates to the manufacture of pottery. They tell us that women produce all the earthenware, that they get their clay and other ingredients in such and such localities; we also learn precisely how they moisten, mix, mold, and fire the clay in order to achieve a viable utensil.⁸

Again, Schweinfurth saw Mangbettu burden baskets, which reminded him of the Thuringian *Kiepen*, slung as they were from

⁸ Georg Schweinfurth, *The Heart of Africa* (London, 1874), II, 60 f.; *idem*, *Artes Africanæ* (Leipzig and London, 1875), pls. xvi, xviii; W. Scoresby and Katherine Routledge, *With a Prehistoric People, the Akikuyu of British East Africa* (London, 1910), pp. 97–102.

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the back by forehead- or shoulder-bands. He does not tell us how the baskets are plaited, whereas a generation or so later the veriest tyro, though uninterested in technology, would bandy such technical verbiage as "twilled openwork twine" or "three-rod foundation." The tyro would not approach Schweinfurth as an observer, but his eyes had been opened to the potentialities of basketry for ethnographic research. An American technologist, Otis T. Mason, had in the interim produced a standard work⁹ classifying the weaves found in the collections of the U.S. National Museum, had determined the distribution of techniques in the New World, and had referred to striking parallels in other parts of the globe. The conditions favoring certain processes had been clarified and, as in most such studies, the alternatives of genetic relations between distant groups or independent evolution had been presented or implied. What does it signify if "wrapped" basketry turns up in Southern Arizona, in archaeological sites of the Mississippi Valley, and in the Andaman Islands? Why are Tierra del Fuegian baskets coiled in a manner strikingly suggestive of Asiatic types? Evidently it became obligatory to consider the minutiae of handweaving. But corresponding conclusions were thrust upon ethnographers whenever *any* phase of culture was closely examined. Even a game of chance might bear crucially on the connection between Asiatic and American peoples. In short, on principle the science of ethnography could not neglect any trait, no matter how trivial apparently, that human groups handed down from generation to generation.

Another conclusion had to be rediscovered. The early theorists had not confined their studies to primitive tribes, but drew upon ancient Egypt and China, classical antiquity, medieval and post-medieval Europe as sources. They realized that Culture formed an indivisible whole; its significant features could be ascertained just as well in the most complex as in the rudest societies, in some respects better, since the complex societies offered written documents and datable events. It was, from all points of view, artificial to segregate the non-literate from the literate peoples. Ethnography must turn into a study not only

⁹ Otis T. Mason, "Aboriginal American Basketry," USNM, *Report for 1902* (Washington, 1904).

of all primitive cultures, but of all cultures past and present, must integrate findings among “savages” with those of prehistory and of the cultural researches among all civilizations. Even though the individual may control only a tiny corner of this vast field, he must be cognizant of its essential unity and continuity.

As ethnography matured, it further became obvious that as a science it must attain objectivity, the same quality that distinguishes the “historical-minded” historian from the partisan chronicler. Such objectivity was easy enough so long as the ethnographer investigated tools and implements, but became difficult in contemplating exotic practices, beliefs, artistic productions,—in short, *values*. The temptation is very great to view these aspects of primitive or alien cultures ethnocentrically. Even far into the nineteenth century, scholars were unable to rid themselves of the notion that the norms of Victorian Europe were absolutely supreme and that any culture must be graded in proportion as it approached the ideals of contemporary Western Europe. So able a writer as Lord Avebury (Sir John Lubbock) was forever expressing indignation over the “revolting” practices of savages, whom he found “almost entirely wanting in moral feeling,”—a conclusion he left unaltered in the 1911 edition of a work first published in 1870.¹⁰

Two comments seem apposite. In the first place, even by our norms, behavior that strikes us as outrageous on closer scrutiny reveals wholly unexpected psychological correlates. It is shocking that the Eskimo will abandon an aged parent. However, the act does not involve the callous brutality it suggests at first blush. The elders simply cannot keep up the pace required for the survival of the household, and themselves beg to be left behind against the remonstrances of their children, lest everyone perish. Such instances have made us wary about ethical verdicts on the strange ways of “savages” or foreigners. But, secondly, the ethnographer, *qua* ethnographer, has no absolute norms for judging good and evil. As in the Eskimo instance, he will always correct misconceptions concerning the facts, and the truth will revise the uninformed judgment of fair and intelligent laymen. But he has

¹⁰ Lord Avebury, *The Origin of Civilisation and the Primitive Condition of Man* (London, 1911), esp. pp. 409–431.

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no professional competence to establish ethical canons. As a human being, he merely hopes that his professional knowledge will create a greater tolerance of spirit.

Because as a specialist the ethnographer accepts *all* cultures, some members of the guild have been sorely troubled. Must they, then, accept Nazi Germany or Soviet Russia? The difficulty is a purely semantic one. Ethnography "accepts" Hitlerism or Stalinism as the zoölogist "accepts" the skunk, the rattlesnake, the mosquito, the turtle-dove, and the elephant. All of them are parts of the universe, hence science must study them; their relative chastity, benevolence, and malevolence simply do not enter.

This elimination of value judgments does not do away with the study of values, of course. That certain societies prize, above all, martial valor while others exalt the arts of peace is an important fact, the relevant attitudes constituting diagnostic features of the associated cultures. As Kroeber puts it, the science of culture deals with "values as natural phenomena occurring in nature."¹¹

A philosopher unusually conversant with ethnographic literature has suggested that a science of culture which deprecates the creation of ethical norms thereby confines its efforts to the descriptive level. Nothing could be farther from the truth. As has been hinted above, out of the raw factual material there arise, spontaneously, efforts at interpretation. As in biology, the mere phenomena of geographical distribution evoke an indefinite number of questions. In part they precisely parallel those familiar in other disciplines. What is the significance of cultural parallels? Are they the result of genetic affinity? Are they due, in popular parlance, to similar causes operating in similar circumstances? Have, perchance, diverse causes led to convergent developments? How can we distinguish between homologous and analogous resemblances? Instead of the cruder hit-and-miss correlations often suggested on intuitive grounds, E. B. Tylor long ago proposed a painstaking recourse to statistics; and his

¹¹ A. L. Kroeber, *The Nature of Culture* (Chicago, 1952), p. 137. Cf. David Bidney, "The Concept of Value in Modern Anthropology," in *Anthropology Today* (Chicago, 1952), pp. 682-699.

pioneer effort has found followers in the writings of G. P. Murdock and Harold E. Driver—irrespective of differences of interpretation as to specific points.

To summarize, the development of scientific ethnography conforms in broad outline to the formulation by Ernst Mach of all scientific development: The initial adaptation of thinking to the facts has been followed by the adaptation of thoughts to one another. Or, put in other words, observation has been followed by integration.

At this point it may be well to consider the kind of integration so far achieved and likely to be attained in the future. If it holds true generally that no branch of learning is under obligation to ape the procedures of any other, this may be asserted most emphatically with reference to the search for ultimate explanations. For that reason it strikes me as a momentary relapse into unwarranted subservience to an older discipline when Professors Kroeber and Kluckhohn in their recent treatise on *Culture* wistfully contrast the theory of gravitation with the lack of a "full theory of culture" and declare that "we have plenty of definitions but too little theory."¹² Certainly we have too little *sound* theory, but I doubt whether even physics can boast of a complete theory; and because cultural phenomena are incomparably more complex than those of physics, because they have come to be studied at a much later period, it seems unreasonable to expect the same measure of integration either now or in the future. As Thurnwald aptly contends, the web of socio-cultural phenomena is too intricate to permit a resolution into all its components without residue, yet we must attempt to discover all possible connections and determinants.¹³

What is the nature of such interpretations? In ethnography, integration has been historical or processual, though as a rule neither type has excluded the other. When investigators have stressed the historical point of view, more or less as biologists did in the era of phylogenetic speculation, they have not neglected the factors that make for cultural change. This is

¹² Kroeber and Kluckhohn, *op. cit.*, p. 181.

¹³ Richard Thurnwald, *Die menschliche Gesellschaft in ihren ethnozoologischen Grundlagen* (Berlin and Leipzig, 1935), IV, 321.

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equally true of those generalizers who believed in uniform stages of evolution the world over and of the more recent scholars who postulate development along several distinct lines. For example, in 1877 Lewis H. Morgan outlined a series of statuses from lowest savagery to civilization and regarded progress as "substantially the same in kind in tribes and nations inhabiting different and even disconnected continents"; but he was by no means content with tracing the sequence of events. Thus, in strongly maintaining that patrilineal descent was everywhere preceded by matriliney, he offered a cause for the shift, viz. the rise of property along with the desire to transmit it to one's own children rather than to matrilineal kinsfolk.¹⁴ Father Wilhelm Schmidt, one of Morgan's most determined critics, opposes to Morgan's unilinear system a multilinear scheme of development and rejects the theory of matriliney as a necessary antecedent of patriliney, assigning both rules of descent to originally distinct specializations of the primeval state of sexual equality. But in order to account for the rise of either he has recourse to the economic preponderance of the female and the male sex, respectively.¹⁵

The double interest in a chronological arrangement of the facts and in an explanation of *why* they appeared conjointly or in the succession discovered characterizes some of the most recent theories. V. Gordon Childe sees in the invention of the wheel one consequence of metal tools in the hands of skilled craftsmen; and he shows how the substitution of iron for bronze democratized metal work, inaugurating a series of technological improvements that culminated 2,000 years ago in "all major manual tools of industry and agriculture" known in the Mediterranean area.¹⁶ J. H. Steward consciously joins sequential and functional considerations. He traces the development of higher civilizations in the Old and New World to the invention of irrigation. This produced in both cases an increase of population; and "a priestly class developed because increasing popula-

¹⁴ Lewis H. Morgan, *Ancient Society* (New York, 1877), Part I, chap. 1, and Part II, chap. 14.

¹⁵ Schmidt and Koppers, *op. cit.*, pp. 210, 264 ff.

¹⁶ V. Gordon Childe, *Archaeological Ages as Technological Stages* (Huxley Memorial Lecture, London, 1944).

tions, large irrigation works, and greater need for social coordination called upon religion to supply the integrating factor.”¹⁷

Given the diversity of cultural phenomena, it is hardly to be expected that all of them should be at any one time amenable to the same degree of analysis. Apart from language, which represents a highly specialized type of socially transmitted features, social organization probably represents the department of culture within which functional relationships have been most satisfactorily defined. Such relationships have often been alleged, probably from the beginnings of ethnographic observation in its pre-scientific stage, but with little attempt to submit the proposed correlations to a rigorous test. A famous lecture by E. B. Tylor, going back to 1888, marks an epoch, irrespective of the acceptability of some of his conclusions, for it represents an effort at exact statistical verification.¹⁸ For example, Tylor asks whether the frequent association of a taboo against speaking with one's mother-in-law and the practice of settling with the wife's parents is due to mere chance or to an organic nexus of the two customs and arrives at a positive conclusion, though explicitly recognizing the existence of more than one determining factor. The most ambitious attempt to follow in Tylor's footsteps is a book by Murdock,¹⁹ in which numerous correlations are defended on a statistical basis. Though a recent critic considers Murdock's correlations too high, he admits that “their value taken collectively is beyond question” and wholeheartedly concurs in the use of exact treatment of our observational material.²⁰

It should not be supposed that sound interpretation in this field hinges solely on specific mathematical techniques. Even without them Radcliffe-Brown, Lévi-Strauss, Eggan²¹—to mention some conspicuous examples—have shown that certain phe-

¹⁷ J. H. Steward, “Cultural Causality and Law: A Trial Formulation of the Development of Early Civilizations,” *American Anthropologist*, LI (1949), 1–27.

¹⁸ E. B. Tylor, “On a Method of Investigating the Development of Institutions,” *RAI, Journal*, XVIII (1889), 245–269.

¹⁹ George Peter Murdock, *Social Structure* (New York, 1949).

²⁰ Harold E. Driver, “Statistics in Anthropology,” *American Anthropologist*, LV (1953), 42–59.

²¹ A. R. Radcliffe-Brown, *The Social Organization of Australian Tribes* (Sydney, 1931); Claude Lévi-Strauss, *Les Structures élémentaires de la parenté* (Paris, 1949); Fred Eggan, *Social Organization of the Western Pueblos* (Chicago, 1950).

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nomena of social structure are intimately related, which of course, as Tylor already realized, does not mean a simple 100 per cent causal nexus. The results are due to a rigorous definition of concepts and a broad comparative approach. None of these writers would claim to have found a full theory of social structure, but as Eggan remarks: "Generalizations do not have to be universal in order to be useful."²² In the processual sphere the progress of ethnography lies in the ever-increasing and increasingly better-founded determination of functional relationships between descriptively isolable elements,—in other words, in constantly detecting more and more organic bonds between apparently disparate phenomena. A single formula for the cultural universe, a simple solution of its enigmas, would prove a snare and a delusion.

On the historical side, ethnography has assiduously determined an infinitude of special intertribal relationships and sequences. In broad outline, at least, the main steps have been traced by which the more complex civilizations of the Old and the New Worlds, respectively, have been built out of simpler beginnings. The whole story will never be known, but every synthesis of field discoveries with archaeological research marks another step in the scientific progress of the discipline.

²² Eggan, *op. cit.*, p. 8.

Envoi

In 1947 Robert H. Lowie was presented the Viking Medal at the Albuquerque meeting of the American Anthropological Association. He responded to the award in approximately the following words.

"We Crow Indians *love* to tell our coups in public. However, we know that stark self-praise would ruffle Anglo-Saxon sensibilities. On the other hand, we do not want to fall into the opposite extreme. *That* would expose us to the charge of affecting modesty.

"What, then, shall a prudent Crow do? I will try to put it into the stylistic phraseology of my tribe: That I was the obvious and inescapable choice, I will *not* say. That in choosing me you committed a gross error of judgment, I will *not* say. I will say *nothing*,—that is, nothing but what a Crow says in response to a great supernatural blessing: a deep-felt "ahô, ahôkacira! Thank you! Thank you very much!"

APPENDIX *Syllabus of Seminar on Work of Robert H. Lowie*

IN 1956, SIX YEARS AFTER PROFESSOR LOWIE HAD RETIRED as Chairman of the Department of Anthropology at the University of California (Berkeley), he was invited to give a seminar on his own work. It was an assignment which amused and pleased him. He undertook it with his customary thoroughness and objectivity. The syllabus that he prepared is included here as a guide to the intellectual climate of the period in anthropology during which Lowie was trained and worked. It may also serve as a guide to future seminars on Lowie's place in anthropology. In view of the extensive coverage of his works in the accompanying syllabus, his bibliography has not been included in this volume. Those interested in consulting a complete list of his writings are referred to Dr. Paul Radin's obituary of Robert H. Lowie in the *American Anthropologist*, LX, No. 2, Part 1 (April, 1958), 358-375, and to *Robert H. Lowie, Ethnologist: A Personal Record* (Berkeley and Los Angeles: University of California Press, 1959).

INTRODUCTION

The theme of the seminar is the work of Robert H. Lowie in historical perspective. This means that quite as much attention will be paid to predecessors and contemporaries. No understanding of American anthropology is possible without a comprehension of Franz Boas's life work. Students are therefore required first of all to familiarize themselves with his points of view in cultural anthropology. This is best done by reading relevant essays in his *Race, Language and Culture* (New York, 1940); the chapter devoted to him in Lowie's *The History of Ethnological Theory* (New York,

1937) will aid in directing attention to Boas's main ideas; for a fuller treatment of Boas's work in the several sub-disciplines, see M. J. Herskovits, *Franz Boas* (New York, 1953).

Boas's oldest living student, A. L. Kroeber, has published a collection of his own articles, with introductory commentary, under the title *The Nature of Culture* (Chicago, 1952).

Lowie's contemporaries at Columbia were A. A. Goldenweiser, A. B. Lewis, Edward Sapir, Frank G. Speck, and Paul Radin. On the first four, the obituaries in the *American Anthropologist* should be consulted, further Goldenweiser's *History, Psychology, and Culture* (New York, 1933) and Sapir's *Selected Writings* (Berkeley, 1949), posthumously printed under the editorship of David G. Mandelbaum. Radin is still living and active. Of his earlier work the most representative publications are probably *Primitive Man as a Philosopher* (New York, 1927); "A Sketch of the Peyote Cult of the Winnebago: A Study in Borrowing" (*Journal of Religious Philosophy*, 7:1-22, 1914); "Literary Aspects of North American Mythology" (*Canada, Geological Survey, Museum Bulletin* 16, 1915); and the field report on *The Winnebago Tribe* (BAE 37, 1923). Typical of Radin's recent output are his *Primitive Religion* (New York, 1937); *Gott und Mensch in der primitiven Welt* (Zürich, 1953); and *The Evolution of an American Indian Prose Epic* (Basel, 1954).

The more recent trends in American anthropology can be best gathered by perusal of the *American Anthropologist* and the *Southwestern Journal of Anthropology*. In 1950 Lowie attempted to summarize the situation for a German audience in Hamburg, the lecture being subsequently printed as "Gegenwartsströmungen der amerikanischen Völkerkunde" (in *Mitteilungen aus dem Museum für Völkerkunde in Hamburg*, Hamburg, 1951, pp. 7-27). A briefer summary by him is to appear soon in *Sociologus*.

For the older British anthropologists, such as Tylor and Rivers, see Lowie's *The History of Ethnological Theory* and A. C. Haddon, *History of Anthropology* (London, n.d.). After Rivers the two dominant figures have been Br. Malinowski and A. R. Radcliffe-Brown. The latter's writings can be conveniently studied now under the title *Structure and Function in Primitive Society* (Glencoe, Ill., 1952). Malinowski's significance for the development of modern British field work is stressed in Aubrey Richards' "The Development of Field Work Methods in Social Anthropology" (in F. C. Bartlett *et al.*, editors, *The Study of Society* [London, 1939], 272-316). For the most recent developments, see J. H. M. Beattie, "Contemporary Trends in British Social Anthropology" (*Sociologus*, n.s. 5:1-14, 1955) and the articles by Firth, Fortes, and Schapera cited in the bibliography.

In the German culture sphere, Father Wm. Schmidt and Richard Thurnwald, both somewhat younger than Boas, but older than Kroeber, exerted a wide influence in diverse ways. The obituaries in *Anthropos* and *AA* roughly indicate their interests and aims. Schmidt's own exposition may be

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followed in his and Wm. Koppers' *Völker und Kulturen* (Regensburg, 1924). It is important to note the recent defection from some of his basic views on the part of his disciples (see Robert Heine-Geldern in *Yearbook of Anthropology*, New York, 1955, pp. 620 ff.). For Thurnwald, both a field worker and a theorist combining sociological with ethnological interests, typical publications are *Forschungen auf den Salomon-Inseln und dem Bismarck-Archipel* (Berlin, 1912); *Die menschliche Gesellschaft* (Berlin and Leipzig, 5 vols., 1931-35); and "Pigs and Currency in Buin" (*Oceania*, 5:119-141, 1934).

In France, professional anthropologists for a long time neglected theoretical ethnology, which, however, was worthily cultivated by E. Durkheim in *L'Année sociologique*, founded by him in 1898, and by his disciples, notably Marcel Mauss (cf. Seth Leacock, "The Ethnological Theory of Marcel Mauss," *AA* 56:58-73, 1954; Claude Lévi-Strauss, in George Gurvitch and W. E. Moore, editors, *Twentieth Century Sociology* (New York, 1945), 503-537; *idem*, "Introduction à l'oeuvre de Marcel Mauss," in Marcel Mauss, *Sociologie et anthropologie* (Paris, 1950), 1-52. L. Lévy-Bruhl, a philosopher, likewise utilized vast bodies of ethnographic material for interpretative purposes. Mauss, Lévy-Bruhl, and Paul Rivet collaborated in founding (1926) the *Travaux et Mémoires de l'Institut d'Ethnologie*, thereby creating an outlet for ethnographic and linguistic publications. For recent developments in France, consult Georges Balandier, in *Yearbook for Anthropology*, 1955, pp. 525-540.

In determining the historical position of any particular American ethnologist in Lowie's generation, it is important to consider (a) the influence of Boas upon him; (b) influences from other sources, whether anthropological, sociological, psychological, or what not. Corresponding comparisons will aid in the determination of his scientific personality. In assessing scientific achievement it is necessary to ask what facts or interpretations a particular individual contributed to the previously acquired stock. Historically significant is his influence on others, a point not at all necessarily coinciding with the worth of his contributions. It may well be that things he regarded as subordinate proved potent influences, while what he put first in his writings remained totally ineffective.

COURSE ASSIGNMENTS

There will be no term paper, but a series of readings for each session. All students are to read the same assignment each time and to present their reactions each week in a brief paper not necessarily over 2-3 type-script pages in length.

Naturally it is not expected that students will read literally all of all the publications cited below except where specific pages are indicated. They are expected to *inspect* the works cited and to read selectively. So far as

Lowie's writings are concerned, those he regards as contributions are certain articles and parts of monographs or of books rather than his books, to wit, the following:

- "Exogamy and the Classificatory Systems of Relationship" (AA 17:223-239, 1915).
- "Family and Sib" (AA 21:28-40, 1926).
- "A Note on Relationship Terms" (AA 30:263-267, 1928).
- "Hopi Kinship" (AMNH-AP 30:365-367, 379-383, 1929).
- "The Omaha and Crow Kinship Terminologies" (ICA 24:103-108, 1930).
- "Plains Indian Age-Societies: Historical and Comparative Summary" (AMNH-AP 11:881-984, 1916)
- "A Note on Aesthetics" (AA 23:170-174, 1921).
- The Crow Indians* (New York, 1935), 104-118.
- "Studies in Plains Indian Folklore" (UC 40:1-28, 1942), 19-26.
- "Reflections on the Plains Indians" (*Anthropological Quarterly* 3:63-86, April, 1955).

BOOKS

Of the books only the first four are prescribed reading, but for the aid of students all are listed, with references to critical comments:

1. *Primitive Society* (New York, 1920; reprinted with very few changes in 1947), 463 pp.; the reprint contains addenda on pp. 442 f. and a new preface.

This is the only one of Lowie's books that proved effective, though definitely not his own favorite. E. (not A.) Métraux translated it as *Traité de sociologie primitive* (Paris, 1935); a Japanese rendering by Tadao Kawamura appeared, allegedly there was also a Chinese one.

An interesting comment by a layman will be found in Justice Oliver Wendell Holmes' correspondence (M. de Howe, *Holmes-Pollock Letters*, Cambridge, 1942, 2:59).

Reviews by specialists include:

- A. L. Kroeber, AA 22:377-381, 1920.
- W. H. R. Rivers, AA 22:278-283, 1920.
- Edward Sapir, *The Freeman*, June 30, 1920, pp. 377-379.
- , *The Nation*, July 10, 1920, pp. 46 f.
- Wilhelm Schmidt, "Die Abwendung vom Evolutionismus und die Hinwendung zum Historizismus in der Amerikanistik," *Anthropos* 16-17:487-493, 1921-1922.
- R. R. Marett, *London Mercury*, Feb. 1922.
- Elsie Clews Parsons, *The New Republic*, Nov. 3, 1920.
- A. G. Keller, *Yale Law Journal*, Nov. 1920, pp. 104 ff.
- 2. *Social Organization* (New York, 1948), 465 pp.
- For review, see: G. P. Murdock, AA 51:298 ff., 1949.
- 3. *Primitive Religion* (New York, 1924, 346 pp.; a second edition in 1948

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runs to 382 pp., containing as new pp. 321–337, 348–363, and the preface). Permission was granted for a Japanese translation.

Reviews include:

Elsie Clews Parsons, *AA* 27:560 ff., 1925.

C. C. Uhlenbeck, *Museum, Maanedblad voor Philologie en Geschiedenis*, Jan. 1926, p. 100.

———, *Internationales Archiv für Ethnographie*, 26: Heft 4–6:16 f.

A. A. Goldenweiser, *New York Tribune*, Sept. 21, 1924.

———, *The Nation*, Dec. 31, 1924.

Frederick Starr, *Unity*, Feb. 1, 1926.

R. Pettazzoni, *Studi i Materiali di Storia delle Religioni*, II, 287 f., 1926.

L. K. Ananthakrishna Iyer, *Current Thought* (Madras, Sept. 1925), 556 ff.

Hartley Burr Alexander, *Saturday Review of Literature*, Nov. 1, 1924.

4. *The History of Ethnological Theory* (New York, 1937), 296 pp.

Paul Kirchoff's Spanish translation appeared as *Historia de la etnologia* (Mexico, 1946).

For reviews, see:

J. M. Cooper, *AA* 41:132, 1939.

Elsie Clews Parsons, *JAFL* 53:78 ff., 1940.

A. C. Haddon, *Nature*, July 2, 1938, p. 5.

M. F. Ashley-Montagu, *Isis*, 29:2:475 ff.

Sture Lagercrantz, *Lychnos*, pp. 426 ff., 1939.

5. *Culture and Ethnology* (New York, 1917), 189 pp.

For review, see: B. Laufer, *AA* 20:87–91, 1918.

6. *The Origin of the State* (New York, 1927), 117 pp.

For reviews, see:

Robert Redfield, *American Journal of Sociology*, Jan. 1928.

Nicholas J. Spykman, *Yale Law Journal*, Feb. 1928, pp. 543 ff.

Richard Thurnwald, *Zeitschrift für Völkerpsychologie und Soziologie*, 5:70 ff., 1929.

Harold J. Laski, *The Nation*, Dec. 7, 1927, pp. 655, 658.

C. C. Uhlenbeck, *Intern. Archiv f. Ethnographie*, 28:155, 1927.

W. Koppers, *Anthropos*, 23:376–377, 1928.

7. *Are We Civilized?* (New York, 1929), 306 pp.

For review, see: B. Laufer, *AA* 32:161–165, 1930.

8. *An Introduction to Cultural Anthropology* (New York, 1924, 365 pp.; the second edition, 1940, runs to 584 pp., of which 342–540 are new).

For review, see: Frank G. Speck, *AA* 37:686–689, 1935.

E. Métraux translated the first edition under the title *Manuel d'anthropologie culturelle* (Paris, 1936); the second edition was translated into Spanish by Javier Romero: *Antropologia cultural* (Mexico, 1947).

9. *The Crow Indians* (New York, 1935), 350 pp.

For review, see: Clark Wissler, *AA* 38:654 ff., 1936.

10. *The German People; a Social Portrait to 1914* (New York, 1945), 143 pp.

For review, see: E. H. Ackerknecht, AA 48:455 ff., 1946.

11. *Toward Understanding Germany* (Chicago, 1954), 396 pp.

For reviews, see:

D. G. Mandelbaum, in *Yearbook of Anthropology* (New York, 1955), 216 ff.
Hildegard Feick, *Sociologus*, 5:90 ff., 1955.

Hans J. Morgenthau, *Chicago Tribune*, May 5, 1954.

Helmut Lindermann, *Stuttgarter Zeitung*, Aug. 27, 1954.

Karl O. Paetel, *Das Historisch-Politische Buch*, Aug. 1954.

12. *Indians of the Plains* (New York, 1954), 222 pp.

For reviews, see:

John Collier, *Saturday Review*, Oct. 16, 1954.

Fred Eggan, AA 57:1309-1310, Dec. 1955.

Appraisal

For a general appraisal of Lowie, see:

Alexander A. Goldenweiser, "Recent Trends in American Anthropology" (AA 43:151-163, 1941).

Claude Lévi-Strauss, "Social Structure," in A. L. Kroeber, *Anthropology Today* (Chicago, 1953), pp. 544 f.

Wilhelm Schmidt, *Der Ursprung der Gottesidee* (Münster in Westfalen), 1:782-790, 1926.

Answering the Queries after the weekly readings assignment is wholly optional. Students are permitted to discuss any ideas suggested by the material perused.

READINGS: 1ST AND 2ND WEEKS

Franz Boas, *Race, Language and Culture* (New York, 1940), pp. 242-323, 344-355, 379-383, 397-406, 425-436, 491-502, 546-563, 639-647.

Robert H. Lowie, *The History of Ethnological Theory* (New York, 1937).

Edward Sapir, *Selected Writings* (Berkeley, 1949), esp. pp. 305-597.

Paul Radin, *Primitive Man as Philosopher* (New York, 1927).

Alexander Goldenweiser, *History, Psychology and Culture* (New York, 1933), esp. pp. 35-55, 121-164.

A. R. Radcliffe-Brown, *Structure and Function in Primitive Society: Essays and Addresses* (Glencoe, Ill., 1952).

Br. Malinowski, *Sex and Repression in Savage Society* (New York, 1927).

A. L. Kroeber, *The Nature of Culture* (Chicago, 1952).

Elsie Clews Parsons, ed., *American Indian Life* (New York, 1922).

READINGS: 3RD WEEK (FIELD WORK)

Inspect the following field reports published by Lowie's immediate predecessors with an eye to field methods employed and mode of presentation in print.

Appendix

- W. Bogoras, *The Chukchee* (AMNH-Mem. X, Leiden, 1909).
F. Boas, *The Central Eskimo* (BAE-R 6:409-669, 1888).
R. B. Dixon, *The Northern Maidu* (AMNH-Bull. 17:33-346, 1902).
James Teit, *The Thompson Indians of British Columbia* (AMNH-Mem. II, 1900).
A. L. Kroeber, *The Arapaho* (AMNH-Bull. 18, 1902, 1904, 1907).
———, *Ethnology of the Gros Ventre* (AMNH-AP 1:141-281, 1908).
P. E. Goddard, *Life and Culture of the Hupa* (UC 1:1-88, 1903).
Karl von den Steinen, *Unter den Naturvölkern Central-Brasiliens* (Berlin, 1894).
A. C. Haddon *et al.*, *Reports of the Cambridge Anthropological Expedition to Torres Straits*, Vols. IV, V, VI (Cambridge, 1902, 1904, 1908).
W. H. R. Rivers, *The Todas* (London, 1906).
Clark Wissler, *Material Culture of the Blackfoot Indians* (AMNH-AP 5:1-175, 1910). *Idem* and D. C. Duvall, *Mythology of the Blackfoot Indians* (same series, 2:1-163, 1908).
George A. Dorsey and Alfred L. Kroeber, *Traditions of the Arapaho* (FMNH-PAS 5, Chicago, 1907).
Inspect:
R. H. Lowie, *The Northern Shoshone* (AMNH-AP 2:165-306, 1909).
———, *The Assiniboine* (AMNH-AP 4:1-270, 1909).
———, *The Crow Indians* (New York, 1935).
———, *Hopi Kinship* (AMNH-AP 30:365-388, 1929) [work done in 1915-16].
———, *Notes on Shoshonean Ethnography* (AMNH-AP 20:191-314, 1924).

4TH WEEK: (FIELD WORK)

Inspect:

- A. L. Kroeber, *Zuñi Kin and Clan* (AMNH-AP 18:39-205, 1917).
A. A. Goldenweiser, *On Iroquois Work* (Canada, Dept. of Mines, Summary Report of the Anthropological Division, Ottawa 1912:12 f.; 1913:464-475; 1914:365-372).
Paul Radin, *The Winnebago Tribe* (BAE-R 37:1-560, 1923) [Note particularly chap. xvi].
———, *The Autobiography of a Winnebago Indian* (UC 16:381-473, 1920).
E. Sapir, *Nass River Terms of Relationship* (AA 22:261-271, 1920).
———, *Hupa Tattooing* (in R. H. Lowie, ed., *Essays in Anthropology presented to A. L. Kroeber*, Berkeley, 1936), 273-277.
F. G. Speck, *Ethnology of the Yuchi Indians* (UPM-AP 1:1-154, 1909).
Inspect:
Leslie Spier, *Havasupai Ethnography* (AMNH-AP 29:81-392, 1928).
———, *Yuman Tribes of the Gila River* (Chicago, 1933).
Gladys Reichard, *Social Life of the Navajo Indians* (New York, 1928).

- Elsie Clews Parsons, *A Pueblo Indian Journal* (AAA-Mem. 32, 1925).
 ———, *Pueblo Indian Religion* (Chicago, 1939).
 Grenville Goodwin, *The Social Organization of the Western Apache* (Chicago, 1942).
 Cora Du Bois, *The People of Alor* (Minneapolis, 1944).
 Julian H. Steward, *Basin-Plateau Aboriginal Sociopolitical Groups* (BAE-Bull. 120, 1938).
 K. N. Llewellyn and E. A. Hoebel, *The Cheyenne Way* (Norman, 1941).
 Br. Malinowski, *Argonauts of the Western Pacific* (London, 1922).

QUERIES: 3RD WEEK

To what extent did Lowie use linguistics as a tool in his Shoshone, Assiniboiné, Hopi, Crow work? What topics did he weight most heavily in each case? Did he use historical sources in his reports? In what respect did he conform to the general pattern used in the United States for field work and reports? How does his approach in the Hopi paper differ from that of the Assiniboiné expedition? To what extent did he anywhere take cognizance of culture change, including recent developments? To what extent did he use biographical material? How does Lowie's field work compare with Speck's, Sapir's, Radin's?

[N.B. No evaluation is called for, only a comparison of field objectives and techniques.]

With Boas's, Kroeber's, Spier's? What differences, if any, appear in the later, as compared with the earlier, Crow field work by Lowie?

QUERIES: 4TH WEEK

What change of pattern in field work appears in such later publications as Du Bois's, Steward's, Goodwin's, Parsons'? To what extent does Edward S. Curtis' Crow work (*The North American Indian*, vol. 4, 1909), say, regarding religion or warfare, harmonize with Lowie's findings? Mischa Titiev's in *Old Oraibi* (PM-P XXII, no. 2, 1944) with Lowie's Hopi data? (Ignore differences of interpretation.) How does Lowie's treatment of the use of wild plants by Shoshoneans compare with Steward's?

Consult the following reviews or critical comments: A. Goldenweiser (AA 15:201 et seq., 1913), with Lowie's reply and partial self-criticism (AMNH-AP 21:53, 56, 57 f., 1917); Ph. Marcou (SAP-J 11, no. 1, 1914) and E. A. Hooton (Current Anthropological Literature, 2:139-142, 1913); C. C. Uhlenbeck (IAE 20:261, 1912); E. C. Parsons (AA 33:232-236, 1931). How does Lowie's field work compare with Malinowski's? Radcliffe-Brown's?

5TH WEEK: AGE-SOCIETIES

Heinrich Schurtz, *Altersklassen und Männerbünde* (Berlin, 1902), 1-83, 151-168.

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A. L. Kroeber, *The Ceremonial Organization of the Plains Indians of North America* (ICA 15, vol. 2:53-63, 1907).

Robert H. Lowie, *The Assiniboine* (AMNH-AP 4:75-98, 1909).

———, *Historical and Comparative Summary* (AMNH-AP 11:877-984, 1916).

Clark Wissler, *General Introduction* (same volume, pp. v-viii, 1916).

A. L. Kroeber, *Review of AMNH-AP 11* (*Science*, 47:241-244, 1916).

QUERIES

Is it correct to call Lowie's treatment a "purely historical one?" What type of "comparative method" is employed? (See: Oscar Lewis, in *Yearbook of Anthropology*, New York, 1955, pp. 259 ff.) To what extent does Lowie depart from Schurtz? From Kroeber? What characteristically Boasian principles of interpretation are applied to the problem in the relevant chapter of *The Assiniboine*? In the final Summary? To what extent have recent writers on social structure dealt with the problem?

6TH WEEK: KINSHIP TERMINOLOGY

Lewis H. Morgan, *Systems of Consanguinity and Affinity* (Smithsonian Contributions to Knowledge, XVII, Washington, 1871), pp. 3-15, 275 ff., 467-510).

A. L. Kroeber, *Classificatory Systems of Relationship* (J.R.A.I., 39:77-84, 1909).

W. H. R. Rivers, *Kinship and Social Organisation* (London, 1914).

Robert H. Lowie, *Exogamy and Classificatory Systems of Relationship* (AA 17:223-239, 329-340, 1915).

———, *Review of Rivers' book* (AA 17:329-340, 1915).

———, *The Kinship Systems of the Crow and Hidatsa* (ICA 19:340-343, 1917 [read 1915]).

———, *Historical and Sociological Interpretations of Kinship Terminologies* (Holmes Anniversary Volume, 1916), 293-300.

———, *Hopi Kinship* (AMNH-AP 30-365 ff., 379-383, 1929).

———, *Family and Sib* (AA 21:28-40, 1926).

———, *A Note on Relationship Terms* (AA 30:263-267, 1928).

———, *The Omaha and Crow Kinship Terminologies* (ICA 24:103-108, 1934).

Paul Kirchhoff, *Verwandtschaftsbezeichnungen und Verwandtenheirat* (ZE 64:45-52, 1932).

Claude Lévi-Strauss, *Les Structures élémentaires de la parenté* (Paris, 1949).

George P. Murdock, *Social Structure* (New York, 1949).

Leslie Spier, *The Distribution of Kinship Systems in North America* (Univ. of Washington, Publ. in Anthropology, 1:69-88, 1925).

E. W. Gifford, *Californian Kinship Terminologies* (UC-PAE 18:1-285, 1922).

QUERIES

How does the treatment of kinship terms and usages in such monographs as Kroeber's *The Arapaho*, Pt. I (AMNH-Bull. 18:9 ff., 1902); R. B. Dixon's *The Northern Maidu* (AMNH-Bull. 17:233 ff., 1905); P. E. Goddard, *Life and Culture of the Hupa* (UC 1:57 ff., 1903); F. G. Speck, *Ethnology of the Yuchi Indians* (UPA-AP 1:68 ff., 1909), compare with, say, Gladys Reichard, *Social Life of the Navajo Indians* (New York, 1928), pp. 74 ff., or Leslie Spier, *Yuman Tribes of the Gila River* (Chicago, 1933), pp. 208 ff.? How do you explain the difference? What is the relation of the indicated pages from Kirchhoff to relevant views of Lowie? How does Spier's treatment resemble, how differ from Lowie's? What is the relation of Rivers' to Lowie's work in this field? Why do you suppose Rivers' booklet, rather than Kroeber's paper in J.R.A.I., stimulated Lowie? What influences appear in Gifford's treatise? Why does Lowie consider the Hopi paper one of his most characteristic publications? How does his kinship work compare with Radcliffe-Brown's? (Remember that throughout the seminar comparison relates to interests, techniques, results, never to comparative evaluation).

7TH AND 8TH WEEKS: (SOCIAL ORGANIZATION, GENERAL)

- R. H. Lowie, *Primitive Society* (New York, 1920).
 ———, *Social Organization* (New York, 1948).
 ———, Notes on the Social Organization and Customs of the Mandan, Hidatsa and Crow Indians (AMNH-AP 21:87–96, 1917).
 A. L. Kroeber, Review of *Primitive Society* (AA 22:377–381, 1920).
 Lewis H. Morgan, *Ancient Society* (New York, 1877).
 E. B. Tylor, On a Method of Investigating the Development of Institutions (JAI 18:245–272, 1889).
 John R. Swanton, The Social Organization of American Tribes (AA 7:663–673, 1905).
 ———, A Reconstruction of the Theory of Social Organization (Boas Anniversary Volume, New York, 1906, pp. 166–178).
 Richard Thurnwald, *Die menschliche Gesellschaft*, 5 vols. (Berlin, 1931–35, 1:1–32; 4:227–323).
 George P. Murdock, *Social Structure* (New York, 1949), general sections.

7TH WEEK: QUERIES

Familiarizing yourself with Boas' ideas on historical laws, cultural patterns, convergence, catchwords, diffusion vs. independent development, secondary association (see, e.g., Boas, *Race, Language and Culture*, New York, 1940, pp. 260–304, 316–323, 379–383), indicate their possible effect on Lowie's interpretations in *Primitive Society*. Does Lowie stress "true" or "false" convergence? Does Lowie admit independent repetition of the same sequences? In what way is his view in regard to the history of the bull-roarer

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anomalous (for him)? Does he agree on this point with Boas (*Handbook of American Indian Languages*, I, Washington, 1911, p. 51)? Is there any indication in *P.S.* that Lowie does not hold the "shreds and patches" theory of culture ascribed to him on the basis of his p. 441? Did Lowie derive the age-area principle from Boas? If not, whence? What major areas of the globe are inadequately dealt with in *P.S.*? Why? Does *P.S.* exclude the possibility of establishing *any* regularities? If not, what regularities are admitted? How does the spirit of Kroeber's review of Vol. XI of the AMNH-AP compare with that of his review of *P.S.*? How does Thurnwald's outlook differ from Lowie's?

8TH WEEK: QUERIES

How does the scope of *Social Organization* differ from that of *P.S.*? Is there any difference in the application of economic and of psychological considerations? How does the treatment of African political institutions differ? What, if anything, is added to the theory of the evolution of the state? [Cf. also on this point *The Origin of the State* and Lowie's Huxley Memorial Lecture (1948).] How do you interpret the closing paragraphs of *P.S.*?

9TH WEEK: (RELIGION)

- R. H. Lowie, *Primitive Religion* (New York, 1924, 1948).
———, The Religion of the Crow Indians (AMNH-AP 25:309–444, 1922).
———, Ceremonialism in North America (AA 16:602–631, 1914).
———, *The Crow Indians* (New York, 1935), 237–334.
E. B. Tylor, *Primitive Culture* (London, 1871), chaps. 11–18.
Paul Radin, *Primitive Religion, Its Nature and Origin* (New York, 1937).
———, *The Road to Life and Death* (New York, 1945).
W. D. Wallis, *Religion in Primitive Society* (New York, 1939).
Franz Boas, *Race, Language and Culture* (New York, 1940), 312–315.
Wm. Schmidt, *Der Ursprung der Gottesidee* (Münster, 1926).

QUERIES

How does the second edition of Lowie's book differ from the first? How does the orientation of either differ from that of *Primitive Society*? How do Lowie's and Radin's views differ? In what respect can both be said to have been influenced by Boas? How does Lowie's use of psychology differ from Clyde Kluckhohn's in *Navaho Witchcraft* (PM-P XXII, no. 2, 1944)? To what extent does the book go beyond the then established patterns of ethnological research in the U.S.? How does Lowie differ from Father Schmidt?

10TH AND 11TH WEEKS: (MYTHOLOGY AND FOLKLORE)

- Franz Boas, *Race, Language and Culture*, pp. 397–490, 503–516.
———, *Tsimshian Mythology* (BAE-R 31, 1916).

- Franz Boas, Northern Elements in the Mythology of the Navaho (AA 10:371-376, 1897)
- Robert H. Lowie, Myths and Traditions of the Crow Indians (AMNH-AP 25:1-308, 1918), 1-13; and inspect footnotes.
- , Studies in Plains Indian Folklore (UC 40:1-28, 1942).
- , Observations on the Literary Style of the Crow Indians (*in* Thurnwald Festschrift, Berlin, 1950), 271-283.
- , A Crow Tale (*Anthropological Quarterly*, 2:1-22, 1954).
- , The Test-Theme in North American Mythology (JAFL 21:97-148, 1908).
- , *The Crow Indians*, pp. 104-171.
- , The Crow Language; Grammatical Sketch and Analyzed Text (UC 39:1-142, 1942).
- Erminie W. Voegelin, Kiowa-Crow Mythological Affiliations (AA 35:470 ff., 1933).
- Paul Radin, *The Evolution of an American Prose Epic* (Basel, 1904).
- , "Winnebago Hero Cycles: Study in Aboriginal Literature" and "The Culture of the Winnebago as Described by Themselves" (Indiana University Pub. in Anthropology and Linguistics, Memoirs 1 and 2, 1948, 1949).
- , The Basic Myth of the North American Indians (Eranos Jahrbuch, 1949, Zürich, 1950), 359-419.
- , Literary Aspects of North American Mythology (Canada, Dept. of Mines, Museum Bull. No. 16, Anthropol. Series No. 6, 1915).
- Ruth Benedict, Introduction to Zuñi Mythology (Columbia Univ. Contrib. to Anthropology, 21:XI-XLIII, New York, 1935).
- Gladys Reichard, *An Analysis of Coeur d'Alène Indian Myths* (Philadelphia, 1947).
- D. B. Shimkin, Wind River Shoshone Literary Forms: An Introduction (Journal of the Washington Academy of Sciences, Oct. 15, 1947), 329-352.
- Julian H. Steward, Myths of the Owens Valley Paiute (UC 34:355-440, 1936), 356-363.
- Robert Spott and A. L. Kroeber, Yurok Narratives (UC 35:143-256). [Note the *commentaries* on the tales.]
- A. L. Kroeber, A Mohave Historical Epic (Univ. Calif. Anthro. Rec. 11: no. 2, 1951).
- A. H. Gayton and Stanley S. Newman, Yokuts and Western Mono Myths (Univ. Calif. Anthro. Rec. 5: no. 1, 1940).
- Richard Thurnwald, Profane Literature of Buin, Solomon Islands (Yale Univ. Pub. in Anthropol., no. 8, 1936).
- Konrad Theodor Preuss, *Religion und Mythologie der Uitoto* (Göttingen and Leipzig, 1921).

Appendix

QUERIES: 10TH WEEK

Is Lowie interested in obtaining several versions of a myth? What is the Boasian justification for doing so? How do Preuss's mythological interests differ from Boas' and Lowie's? How does Lowie use myths and tales for historical purposes? How does Voegelin's paper compare with Boas' on the Navaho? To what extent did Lowie obtain texts in the original tongues?

QUERIES: 11TH WEEK

How would you group Boas, Kroeber, Lowie, Radin, Benedict, Shimkin, Thurnwald, Reichard as to their study of tales from a literary angle? Characterize the approaches involved.

12TH WEEK: (MATERIAL CULTURE)

R. H. Lowie, *Introduction to Cultural Anthropology*, pp. 13-161.

———, relevant portions of tribal monographs.

———, Review of Vavilov (AA 30:716, 1928).

Berthold Laufer, *Sino-Iranica* (FMNH-PAS XV, no. 3, 1910).

———, The American Plant Migration, I. The Potato (FMNH-PAS, 28, no. 1, 1938).

———, The Reindeer and Its Domestication (AAA-Mem. IV, n. 2, 1917).

———, Methods in the Study of Domestications (*Scientific Monthly*, 25: 251-255, 1927).

Clark Wissler, Material Culture of the Blackfoot Indians (AMNH-AP 5:1-175, 1910).

Erland Nordenskiöld, *Comparative Ethnographical Studies* (Gothenburg, 1919, 1924).

QUERIES

Does Lowie show genuine interest in any phase of material culture? In his theoretical use of data from this division of culture, whose influence seems strongest? How do his distributional interests in this field compare with Nordenskiöld's? Wissler's and Spier's? Is there any indication of geographical correlations in his work? How does his work compare with that of his contemporaries (Sapir, Goldenweiser, Radin)?

13TH WEEK: (ART)

Franz Boas, *Primitive Art* (Oslo, 1927).

Clark Wissler, Decorative Art of the Sioux Indians (AMNH-Bull. 18:231-277, 1904).

A. L. Kroeber, The Arapaho, II (AMNH-Bull. 18:36-229, 1904).

- A. L. Kroeber, *Ethnology of the Gros Ventre* (AMNH-AP 1:158-177, 1908).
 Robert H. Lowie, *Crow Indian Art* (AMNH-AP 21:271-322, 1922).
 ———, *A Note on Aesthetics* (AA 23:170-174, 1921).
 Leslie Spier, *An Analysis of Plains Indian Parfleche Decoration* (Univ. of Washington-PA 1:89-112, 1925).
 ———, *Plains Indian Parfleche Designs* (same series 4:297-322, 1931).

QUERY

To what extent does Lowie conform to and deviate from previous considerations of Plains Indian art? What stimulated the *Note on Aesthetics*? What are the ethnographic suggestions thrown out there? Who has taken cognizance of them?

14TH WEEK: (GENERAL POSITION)

(*Note.* Lowie's general position is often expressed in reviews, mainly in AA and JAFL and naturally in the publications previously cited.)

- R. H. Lowie, *The Methods of American Ethnologists* (*Science* 34:604 f., 1911).
 ———, *On the Principle of Convergence in Ethnology* (JAFL 25:24-42, 1912). (Cf. Goldenweiser, *History, Psychology and Culture*, pp. 35 ff.)
 ———, *Some Problems in the Ethnology of the Crow and Village Indians* (AA 14:60-72, 1912).
 ———, *Psychology and Sociology* (*Amer. Jour. of Sociology*, 21:217-229, 1915).
 ———, *The Buffalo Drive and an Old-World Hunting Practice* (*Natural History*, 23, no. 3, 1923, pp. 280 ff.).
 ———, *Review of H. S. Harrison, Pots and Pans* (AA 31:504 ff., 1929).
 ———, *Hugo Obermaier's Reconstruction of Sequences among Prehistoric Cultures in the Old World* (in Stuart Rice, ed., *Methods of Social Science*, Chicago, 1931), pp. 266-274).
 ———, *Queries* (AA 35:288-296, 1933).
 ———, *Religious Ideas and Practices of the Eurasiatic and North American Areas* (in *Essays Presented to C. G. Seligman*, London, 1934, pp. 183-188).
 ———, *A Note on South American Parallels to Maya and Aztec Traits* (*American Antiquity*, 4:157 f., 1938).
 ———, *A Note on Lapp Culture History* (*Southwestern Journal of Anthropology*, 1:447-454, 1945).
 ———, *American Culture History* (AA 42:409-428, 1940).
 ———, *Cultural Anthropology, a Science* (*Amer. Jour. Sociology*, 42:301-320, 1940).
 ———, *The Transition of Civilizations in Primitive Society* (*Amer. Jour. Sociology*, 47:527-543, 1942).

Appendix

- , Reflections on the Plains Indians (*Anthropological Quarterly*, 3:63–86, 1955).
- , Some Problems of Geographical Distribution (in *South Sea Studies, Felix Speiser Memorial Volume*, Basel, 1951), 11–26.
- , The Heterogeneity of Marginal Cultures (ICA 29, vol. 3:1–7, 1952).
- , Ethnography, Cultural and Social Anthropology (AA 55:527–534, 1953).
- , Alleged Kiowa-Crow Affinities (*Southwestern Journal of Anthropology*, 9:357–368, 1953).
- , Evolution in Cultural Anthropology: A Reply to Leslie White (AA 48:223–233, 1946)

QUERIES

Taking the briefer papers and reviews in chronological order, do you find any change in point of view or emphasis? To what extent is Lowie guilty of sticking to the “Monroe doctrine” some diffusionists have charged Americanists with? Does he at all consider “cultural dynamics?” What have been his views on convergence? (Consider, e.g., *Primitive Society*, pp. 31, 104, 109, 131, 140 f., 254, 433 f.) Does Goldenweiser add anything in his paper to the concept of limited possibilities beyond Boas’ statement (1911) in *Race, Language and Culture*, p. 299? What has been Lowie’s position on “laws?” Which of the three meanings of function (*Yearbook of Anthropology*, 1955), have played a part in Lowie’s work? Does Lowie reject evolution *in toto*? Has he ever attempted a large-scale historical reconstruction? How does his historical point of view compare with Wm. Schmidt’s? Kroeber’s? Boas’? Radin’s? Rivers’? The British social anthropologists’?

15TH WEEK: (HISTORY OF ANTHROPOLOGY)

- Wm. Schmidt, *Völker und Kulturen*, 1924, pp. 3–30.
- Wm. E. Mühlmann, *Geschichte der Anthropologie* (Bonn, 1948).
- Alfred C. Haddon, *History of Anthropology* (London, 1935).
- R. H. Lowie, *The History of Ethnological Theory* (New York, 1937).
- , “Biographical Memoir of Franz Boas” (National Academy of Sciences, *Biographical Memoirs*, Vol. XXIV, no. 9, 1947).
- , “Franz Boas, His Predecessors and His Contemporaries,” *Science*, 97:202 f., 1943).
- , “Some Facts about Boas” (*Southwestern Journal*, 4:69 f., 1948).
- , “Franz Boas” (JAFL 57:59–69, 1944).
- , “Alfred L. Kroeber: Professional Appreciation” (Kroeber Festschrift, Berkeley, 1936), pp. xix–xxiii.
- , “Franz Boas, Anthropologist” (*Scientific Monthly*, 56:182 ff., 1943).
- , “Richard Thurnwald” (AA 56:863–867, 1954).
- Paul Radin, *The Method and Theory of Ethnology* (New York, 1933).

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